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Archives of Medicine

A BI-MONTHLY JOURNAL

DEVOTED TO ORIGINAL COMMUNICATIONS ON MEDICINE,
SURGERY, AND THEIR SPECIAL BRANCHES

EDITED BY
E. C. SEGUIN, M.D.

> S'il est possible de perfectionner l'espèce humaine, c'est dans la médecine qu'il faut en chercher les moyens.
>
> —DESCARTES

ELEVENTH VOLUME.

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EDITORIAL ANNOUNCEMENT.

The announcement of the discontinuance of the ARCHIVES OF MEDICINE, made in its last issue, brought the editor many protests from its former contributors and friends. This, together with the fact that the editor has given up his plan of returning abroad for a prolonged residence, led him to decide to continue the publication of the journal.

As in the past, the ARCHIVES OF MEDICINE will appear every two months, as punctually as possible, and will comprise about one hundred pages, more or less, according to the material on hand.

Its distinguishing feature will be that only original articles and reviews will appear in its pages. The editor believes that there is room in America for such a journal, in addition to the monthly and weekly publications which give abstracts, proceedings of medical societies, and medical news in such an admirable manner.

In the selection of original articles the editor will endeavor to provide for the representation of every department of medicine; no specialty will be given any prominence beyond that which naturally proceeds from the relative activity of workers in the various branches of medicine. Strictly scientific articles on anatomy, physiology, applied chemistry and physics, and even psychology, will, in due proportion to practical subjects, be welcome to its columns. It is the editor's intention to incorporate related cases or observations with the more formal papers, in the department for original articles. This is justified by the intrinsic value of well-recorded cases, and by the example of leading

European periodicals, in which important articles not seldom consist of one or two valuable observations with remarks.

The Editorial Department will continue to be devoted to topics of general interest, either by editorial remarks or by analytical and critical articles.

In the Review Department it will be the editor's aim to secure impartial and fearless reviews. During the five years' existence of the journal, only two or three reviews have called forth remonstrances from the authors of the books criticised; and in a recent instance the reply (?) to a perfectly just criticism consisted only of personal abuse of the reviewer—a proceeding which only confirms the reviewer's position.

As heretofore, illustrations will be freely inserted when necessary to the better understanding of the text and for the more precise record of cases.

During the past year Dr. Royal W. Amidon successfully conducted the ARCHIVES under peculiarly difficult circumstances, and the editor desires hereby to thank him for the faithful and very able manner in which he performed the task.

In conclusion, the editor, in soliciting the further support of the medical profession for this enterprise, will promise that on his part and on that of the publishers, no pains will be spared to make the ARCHIVES OF MEDICINE a first-class medical journal within its peculiar sphere.

VOL. XI., No. 1. FEBRUARY, 1884.

ARCHIVES OF MEDICINE.

Original Articles.

A PSEUDO NEGATIVE TRACE.

BY S. E. POST, M.D.

PHYSICIAN TO THE OUT-DEPARTMENT OF THE NEW YORK INFIRMARY FOR WOMEN AND CHILDREN.

1. There exists a pseudo negative trace.
2. This trace is the inverted image of a positive trace. It is produced by direction of impact against the frame instead of against the diaphragm of the explorer, and with the employment of about one half of the pressure which is in the latter case used.
3. The negative trace of the arterial pulse resembles the trace of the negative pulse of the veins. Both fall while the artery rises and rise while the artery falls. Both fall abruptly and rise in a series of curves. Their difference has not yet been made out.
4. The negative trace of the arterial respiratory curve resembles the trace of the respiratory curve of the veins. Both fall during inspiration and rise during expiration. The arterial differs from the venous trace, however, in the fact that the first reaches its minimum after the commencement of expiration, while the second reaches its minimum previous to the commencement of that act.

The existence of the above phenomenon first appeared to me during an observation of the superficialis volæ in a normal case. This artery lay directly beneath the skin, and its pulsation was apparent to the eye. Upon either side and separated from it by the half of an inch were its accompanying veins; they also could be distinctly seen. The Pond sphygmograph was used with the production of the negative trace, which is shown in fig. 1. This trace was produced when the explorer was so placed that one side of its frame only rested upon the artery, its diaphragm being supported by the adjacent part.

Trace I fell while the artery was rising, and it rose while the artery fell; also an uplifting of the entire instrument could be observed with each impulse from the arterial wall. When the explorer was moved to a position over the artery the usual trace, opposite in character, appeared; and when it was removed from it, toward either side, the needle hung motionless. Trace I required arterial impact, but impact only against the frame. In explanation, it is now proposed that the diaphragm, pressed upward by its apposition to the part, would lose its support with the elevation of the frame, and that consequently, with each impulse, the diaphragm fell. The above principle, once true, is always true. Based upon it, the trace produced by impact against the frame must be opposite in character to that produced by the same impact when applied to the diaphragm, while the accomplishment possible to the feeble superficialis volæ may be repeated by any of the larger available arterial trunks.

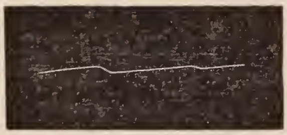

FIG. I.

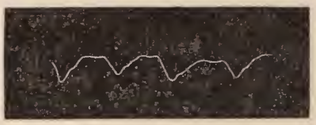

FIG. 2.

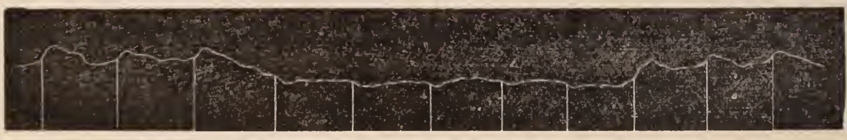

FIG. 3.

These propositions derive importance from their relation to propositions concerning the normal venous pulse or the negative pulse of the veins. As is now known, this pulse consists of an abrupt fall of venous pressure, which is synchronous with the contractions of the ventricles, and which is due to a periodic acceleration of the flow of blood into the heart. Though its existence is said to have been advocated in 1752 by Lamure,[1] rejected in 1759 by Haller,[1] and again advocated by Carson[1] in 1824, by Wedermeyer[2] in 1828, by Weyrich[2] in 1845, by Berthold[1] in 1869, and by Cappie[1] in 1874, its modern interests must be considered to date from the publications of Mosso[3] in 1878 and 1879. This observer first obtained from the jugular vein a tracing which distinctly fell during the elevation of the carotid wall. Following Mosso, Franz Riegel,[4] May 2, 1881, stated that he had observed in dogs a venous pulse which alternated in time with that of the arteries, also that in many healthy men he had obtained a pulse which did not differ from that he had obtained in dogs. He summed up by saying: "There exists a normal venous pulse. This pulse falls during the heart's (ventricle's) systole, and rises during its diastole. It is explained by the fact, that during the systole of the heart (ventricle) the flow of blood from the veins is accelerated, while during the diastole of the heart it is retarded." May 14, 1881, Gottwalt[5] presented a paper entitled "The Normal Venous Pulse." Gottwalt's observations were upon dogs. The vein was dissected from its surroundings, under it was placed a concave tambour, and over it a closely fitting metal plate. Gottwalt's curve consisted of one large wave preceded by two smaller ones. So nearly as he could tell, the second sound of the heart corresponded with the apex of the first smaller wave.

In April 1882, François Franck[6] gave traces taken simultaneously from the jugular vein and from the heart.

The tracing from the vein is similar in form to that given by Riegel. Following the sequence of the cardiogram, however, it is supposed to consist of: 1, a sudden rise and descent at the commencement of the total curve; 2, a second rise and descent; 3, a gradual ascent, and then again a sudden rise, indicating the commencement of a new series. The sudden rise and descent were synchronous with the contraction and relaxation of the auricle, and occurred with equal promptitude when the contractions of the ventricle were arrested by stimulation of the pneumogastric nerve. The second rise accompanied the ventricular systole.

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[1] *Jour. de méd. de chir. et de pharm.*, Jan., 1883, p. 48.

[2] Hermann's "Hdb. der Phys.," Bd. IV., Th. I., S. 331.

[3] Hermann's "Hdb. der Phys.," Bd. IV., Th. I., S. 178.

[4] Riegel: *Berl. klin. Wochenschr.*, May 2, 1881.

[5] Gottwalt: *Pflüger's Arch.*, May 14, 1881.

[6] Franck: *Gaz. hebd.*, April 7, 1882, p. 225.

His explanation is as follows: The sudden rise is due to increased pressure from obstructed flow at the instant of the contraction of the auricle, and the sudden fall to rapidity of the current at the commencement of the diastole, when the blood precipitates itself into the relaxing auricle. The second rise is due to retardation of the current from the loss of the aspiratory influence of the auricle, and the second fall to a new acceleration from relaxation of the ventricle, while the gradual ascent is due to retardation from the gradual repletïon of the whole right heart.

Further communications upon the subject have been made by Riegel[1] since this time, but these are chiefly explanatory and do not add any new facts.

To recapitulate: the negative pulse of the veins consists of a diastolic ascent prolonged and broken into waves, and of a descent which is systolic and abrupt. In the words of Riegel, it is in form and in time opposed to the normal arterial pulse. The above description is also absolutely true for the arterial sphygmogram from frame impact as presented in this paper and as illustrated in fig. 4. It is conclusive, therefore, that the trace thus produced should be eliminated from all considerations relative to the venous curve. A short time ago the writer[2] presented tracings supposed, from their systolic descent, to represent this venous curve. The tracings from the external jugular were from pulsation which was apparent to the eye. With the exception of these the pulsations supposed to have been registered, were not seen, and in each instance the instrument was in a position to have received arterial frame impact; while from a similarity in the number and position of the secondary waves to the number and position of the secondary waves in the inverted arterial trace a similar origin must be inferred. Also the circumstances found to accentuate the supposed venous trace were such as would accentuate a trace from the arterial pulse.

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[1] Riegel: *Deutsches Arch. f. klin. Med.*, May 30, 1882; *Volkmann's Sammlung klin. Vorträge*, March 13, 1883.

[2] *N. Y. Med. Record*, Feb. 17, 1883.

Many of Riegel's tracings also show this likeness between the venous curve and the inverted arterial curve of the part, notably figs. 5; 7, *a* and *b*; 8, *a* and *b*; and 9, *Deutsches Arch. f. klin. Med.*, May 30, 1882, and fig. 2, *Volkmann's Sammlung*, March 13, 1883. In the presence of this similarity we should have evidence that the possibility of frame impact had been avoided.[1]

Other interpretations also require to be reconsidered from the stand-point of this possibility. Dr. Ebenau[2] recently presented a negative trace as the trace of an arterio-venous aneurism, which was afterward dissected and tied. The instrument was applied over the aneurism, which involved the femoral vein. The Marey sphygmograph was used. His inference was based upon the argument that no other explanation remained for the trace, as the normal venous pulse did not extend so far toward the periphery, while there existed no lesion of the tricuspid valve. His instrument also was in a position to have received arterial frame impact.

More recently Dr. A. T. Keyt[3] has presented negative tracings as representing "The Negative Arterial Pulse," supposed to be a pulse of the tissues adjacent to the artery. The illustrations which accompany this paper refute its theory. Its theory requires aspiration of the skin under the explorer, this aspiration to be dependent upon change in the position of the arterial wall. Its author finds the arterial pulse to consist of two movements, expansion of the lumen of the artery and locomotion of the whole vessel from its bed. Of these movements locomotion only is capable of effecting aspiration—expansion would tend to diminish it. Further, the perivascular tissues are not in a condition of vacuum. They are a mass of freely communicating lymph-spaces upon which the aspiration would primarily be spent. The trace of skin-aspiration by the arterial pulse, then, would equal in amplitude the trace of the arterial pulse minus the influence of expansion and minus the influence of the aspiration of lymph. Compared with the pulse-trace it would be a curve of a limited extent. The author's negative trace is, on the contrary, in some cases an inverted image of his positive trace, while the position of his instrument was such as would favor arterial impact against the frame.

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[1] Gottwalt's observations were upon isolated veins, while Franck's explorer was supported above the surface of the part.

[2] Ebenau: *Berl. klin. Wochensch.*, January 8, 1883.

[3] Keyt: *Boston M. & S. J.*, October 11, 1883.

Not only the sphygmogram but also the cardiogram may be negatively fac-similed by frame impact. An instance is shown in fig. 3. Fig. 3 was obtained from a normal heart, the explorer being applied in the fifth intercostal space. Its transition from positive to negative was effected by moving the loosely adherent skin upward until the lower border of the explorer rested in the centre of the space, and that from negative to positive was effected by its return. That the negative descent was systolic is shown by the spacing of the trace. Systolic sinking of the chest-wall has been noted in the third and fourth intercostal spaces with hypertrophy of the heart.[1] The heart occupies less space during systole than during diastole.[1] The cardiac systole has an aspiratory force, but that force is principally expended upon the lungs, whose volume increases during systole and decreases during diastole with the rhythm of a pulse.[2] The influence of heart-aspiration upon the thorax, therefore, must be slight. This heart also was normal and feeble in its systole rather than otherwise. Further, the diaphragm of the instrument rested upon the fifth rib. The production of the trace by aspiration, hence, was impossible.

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[1] Landois: "Lehrbuch der Phys. des Mensch.," Th. I., S, 98. 1883.

Without moving the explorer, the cardiogram does at times pass from positive to negative under the eye. Franck[3] has published a tracing which was thus obtained. Franck's tracing was obtained during a profound inspiration. He considered it to imply predominating effort at systole from an exaggerated repletion of the heart. Franck's sphygmogram simultaneously taken shows increased height of the percussion stroke, but does not show any systolic irregularity of the heart. Repletion was not present in my case, and further, the negative trace was produced at will by simply altering the position of the instrument so that impact could occur against the frame. Upon the theory of this occurrence, spontaneous change to the negative also can be explained by advancement or recession of the apex from increase or diminution of systolic force.

Fig. 2 is also a cardiogram; it is presented in this connection only as a more perfectly formed trace. It also is to be read from the left to the right. Trace 2 was obtained from a case of mitral stenosis with dilatation of the heart. Cyanosis was marked, and a pulse was present in the external jugular vein. This pulse was distinctly negative.

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[1] Marey: "Trav. du lab.," p. 51, 1875. Franck: "Trav. du lab. de Marey," p. 187, 1877.

[2] Voit: *Ztschr. f. Biologie*, I., S. 390, 1865.

[3] Franck: "Trav. du lab. de Marey," p. 59, 1876.

It fell with the arterial impact. The descent of the cardiogram occurred at the same time. Though repeated attempts were made, a positive cardiogram could not be obtained in this case.

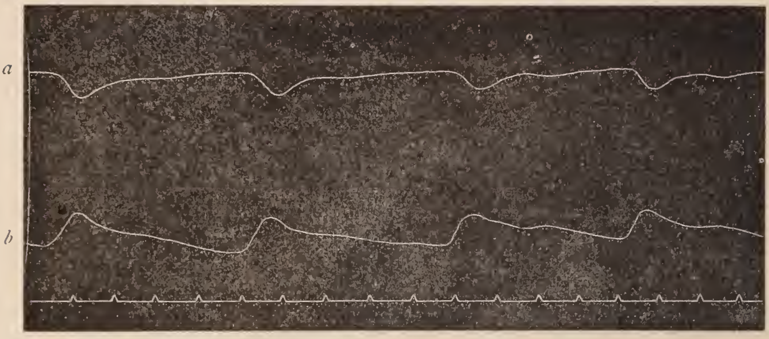

FIG. 4.

a. From the left radial by impact against the frame.
b. Simultaneously trom right radial by impact against the diaphragm of the instrument.

The persistently negative cardiogram has been associated with mediastinitis and the resulting adhesion of the pericardium to the chest. Systolic sinking did not occur in this

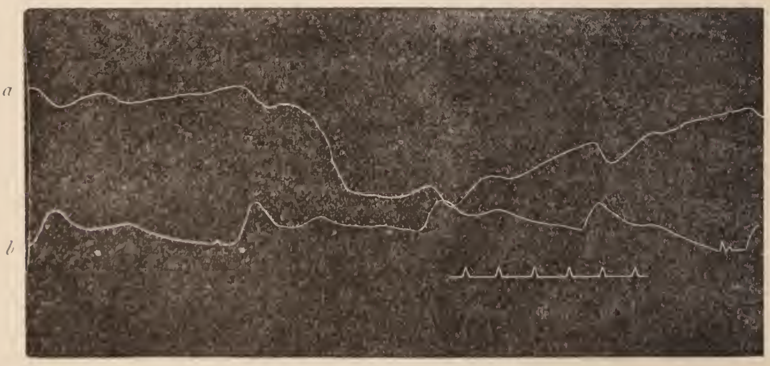

FIG. 5.

a. From left radial by impact against frame.
b. From right radial by impact against diaphragm.

case. On the contrary, a wave of expansion was visible, which covered a considerable area. The explanation of fig. 2 probably lies in this diffuseness of the beat, which would be unable to lift separately the limited surface covered by the diaphragm, and which could produce any trace only when lifting one side of the frame.

The respiratory blood-pressure curve is also capable of falsification by this same method. Fig. 5, *a* and *b*, show simultaneous tracings from the two radial arteries: *b* produced by impact against the diaphragm, and *a* produced by impact against the frame. The single pulse-curve of *a* presents the same opposition to the pulse-curve of *b*, which is shown in fig. 4. The two tracings of fig. 5 present, in addition, lines of respiratory variation in which also *a* opposes *b*. Here, however, we do not find an inverted image, as in the case of the pulse-curve. The negative respiratory line

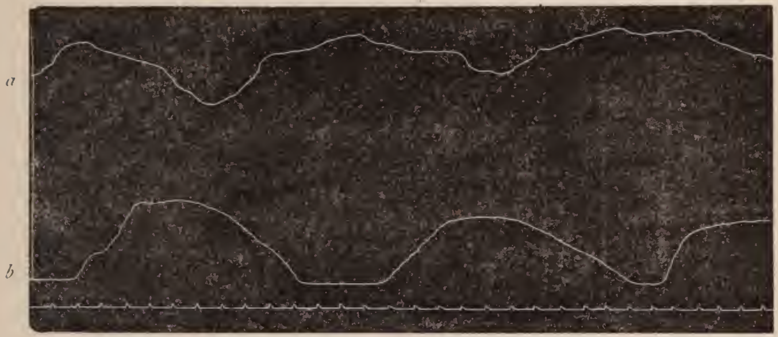

FIG. 6.

a. From the carotid by impact against the frame.
b. From the sternum by impact against the diaphragm.

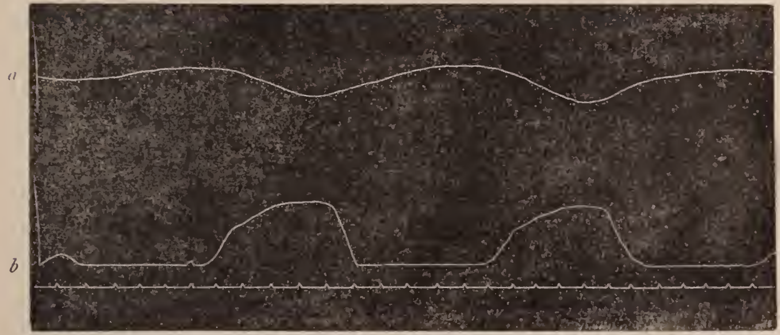

FIG. 7.

a. From the ext. jugular vein by impact against diaphragm.
b. From the sternum by impact against diaphragm.

shown in *a* is apparently exaggerated. It is not really exaggerated, the respiratory line in *b* is repressed, as respiratory variations do not require the pressure which is employed to register the pulse.

The negative respiratory line would be a matter of small importance did it not also simulate a real phenomenon. In normal respiration the arterial blood-pressure rises during inspiration and sinks during expiration, while the venous blood-pressure sinks during inspiration and rises during expiration. The negative trace of the arterial variation will also fall during inspiration and rise during expiration, and hence approach the character of the venous curve. It is not, however, its exact counterpart. The respiratory rise in arterial blood-pressure reaches its maximum after the commencement of expiration.[1] Its negative trace will reach its minimum necessarily at the same period. This occurrence is shown in fig. 6, *a* and *b*, taken simultaneously,—*a* from the carotid artery by frame impact, and *b* from the surface of the thorax by impact against the diaphragm.

The respiratory variation of blood-pressure in the vein, on the other hand, though also falling during inspiration and rising during expiration, possesses a distinguishing mark. It reaches its minimum previous to the commencement of the expiratory act. This peculiarity has been recently considered by Franck.[2] It is rudely represented in trace 7, *a* and *b*, trace *a* being obtained with the explorer over the external jugular, and trace *b* simultaneously by an explorer applied to the thoracic wall. Impact occurred against the diaphragm in both instances.

To conclude, negative traces from impact against the frame are capable of producing confusion in otherwise valuable evidence. A possibility of frame impact should be considered in all deductions relative to a negative trace.

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[1] Foster's "Physiology," p. 344, 1879.

[2] Franck: *Gaz. hebd. de méd. et de chir.*, March 10, 1882, p. 157.

The compound sphygmograph used in these observations was the Keyt, kindly loaned to me by Dr. M. P. Jacobi. Similar negative sphygmograms have been obtained also from the Dudgeon instrument whose diaphragm and pedestal are upon the fashion of the Marey, while its conducting and registering apparatus resembles that of the Pond.

ON ANGIOMA OF THE LARYNX.

By LOUIS ELSBERG, M.D.

PROFESSOR OF LARYNGOLOGY AND RHINOLOGY IN THE NEW YORK POLYCLINIC AND DARTMOUTH MEDICAL COLLEGE.

ANGIOMATA, *i.e.*, true blood-vessel growths, are among the rarest of all intra-laryngeal tumors. Aside from the two cases I am about to relate, I know of but three recorded instances.

CASE 1.—On July 10, 1876, Mr. E. H., a merchant, thirty-seven years old, consulted me on account of an increasing hoarseness which had troubled him from time to time for over six years. Born of healthy parents, and never having been seriously ill in his life, he had contracted during a twelve years' residence in Brooklyn a naso-pharyngeal catarrh which he thought had gradually involved the larynx. During the last two years it has somewhat affected his general health and strength, though with the exception of dyspeptic symptoms all his functions are well performed. Since last spring he lives in New York City, and thinks his catarrh is a little less troublesome ; nevertheless every morning, on rising, the phlegm nearly chokes him, and he must hawk and spit a great deal the whole day. For half an hour after he has thoroughly cleaned out his nose and throat, he finds that his voice is worse than at any other time, but in the course of an hour or two becomes better, and remains nearly normal for several hours ; then it " hoarses up," and becomes more and more husky, until about half an hour after the next clearing out. He coughs very little, and has no pain in any part of his throat or respiratory tract.

The patient is a very energetic man engaged in a business requiring him to use his voice a great deal. While relating the above history of his case (at about 10 o'clock in the morning) his voice sounded but little veiled. Examination of his chest excluded both pulmonary and cardiac trouble, either the one or the other of which the intelligent but rather nervous patient had feared. I found catarrhal congestion and hypertrophy in his nose and throat, blood-vessels abnormally visible on the epiglottis and on the vocal bands, and attached to his right vocal band a round black tumor the size of a pea. There appeared to be a thin dark pedicle adhering lengthwise to the vocal band, extending from the anterior commissure for fully an eighth of an inch along the edge, and the tumor, round though it was, had a portion of its periphery likewise attached to the vocal band so that nearly half an inch of what should have been a free vocal edge was involved. Looking at the laryngoscopical image, as sketched in fig. 1, the clearness of the patient's voice was astonishing to me.

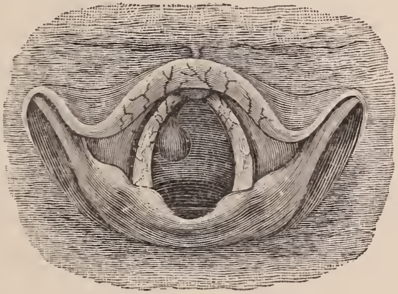

FIG. 1.

Mr. H. bore the application of the mirror remarkably well, and after a little drilling and a few fruitless attempts, I succeeded during his first visit in snaring off the tumor with Gibbs' wire loop. Considerable hemorrhage ensued immediately, which was prolonged by violent coughing, straining, and peculiar expiratory efforts of the now intractable patient.

After twenty minutes or half an hour, the patient allowed me to inject a nebulized solution of persulphate of iron, which in less than five minutes arrested the bleeding. Laryngoscopical examination after this was unsatisfactory. I prescribed inhalation of the nebulized solution for a few minutes every two hours.

The tumor which had adhered to the wire loop when removed was bluish-black and the size of a pea, but it soon lost its dark color and became grayish. Later, on microscopical examination, it showed a fine spongy structure composed of trabeculæ of connective tissue containing blood and blood-vessels, so that the diagnosis of the tumor as *angioma cavernosum* was indubitable.

On the next day the larynx was very painful and there was aphonia, but no more bleeding had taken place. The pedicle, adherent to the right vocal band, and a stump of the tumor were distinctly seen. The inhalation was ordered to be continued every three hours.

July 12th. Patient has slept badly; is feverish; tongue furred, bowels constipated. Aphonia persists, but laryngeal pain has diminished. The stump of the tumor appears shrunken. Prescribed a purge of calomel and jalap; inhalation only three times daily.

From this time on, the patient's voice improved rapidly. Local applications of my solution of iodoform in ether (ʒ ij ad ℥ j) were made in the interior of the larynx, and to the mucous membrane of the pharynx and nose. The inhalation was discontinued after about a week.

Two months later no trace of the tumor was visible. Even the pedicle had disappeared. The patient's voice was perfectly satisfactory, and his catarrh had ceased to be troublesome.

No recurrence has taken place.

Case 2.—J. C. C., æt. twenty-eight, of healthy parentage, presented himself to the New York Polyclinic, on Oct. 15, 1883. As an infant he has had whooping-cough, later diphtheria, and for many years catarrh. His voice was very clear until about five years ago. At that time he was lecturing with Edison's Phonograph ("Palingenophone"), and once gave eight different lectures of one hour each, in one day. On the next morning he could scarcely speak above a whisper. This was the first time in his life that he was hoarse, and, though his voice soon became comparatively clear again, he never got entirely free from an occasional, more or less, slight huskiness. Four years ago he came to New York to study vocal music, and for one year his voice improved very much; but three years ago last June he began to notice an increasing falling off of power and purity of tone; at times he could not sing at all. He then consulted Dr. W. W., who thought his tonsils, which were enlarged, caused the trouble, and removed them. For a time it seemed he was getting better, but by degrees he became worse again. One year ago last August it was only with great effort that he could speak plainly ; under treatment his voice improved, so that he could sing over an octave, but the relief was only temporary. In January, 1883, his vocal instructor, Mr. K., on examining the larynx, discovered a tumor on his right vocal band, the existence of which was afterward confirmed by his physician. At that time he could produce, with an effort on his part, one octave and a half of almost clear tones, and another physician, who examined him with the laryngoscope, said there was no tumor, but that he was suffering from "a partial paralysis of the vocal cords." He was treated with the electric battery, and was assured that the hot months of the coming summer would cure him entirely. But instead of getting better his voice became worse. Since August, he has gone to the Manhattan Eye and Ear Infirmary, the New York Eye and Ear Infirmary, and the clinique of the College of Physicians and Surgeons, and has been examined by a large number of doctors and students.

On admission, his general health was found to be excellent. He is married, and has never had any venereal disease. His conversational voice is distinctly husky, but he can produce single notes of considerable clearness and musical clang, ranging through nearly an octave of a baritone voice. Aside from this condition of his voice, he has, he says, absolutely nothing to complain of. Examination revealed, in addition to catarrhal condition of the nose and throat, a reddish-black, smooth pyriform tumor, attached to the edge of the right vocal band, extending from the anterior commissure back for at least a third of its length, as shown in fig. 2.

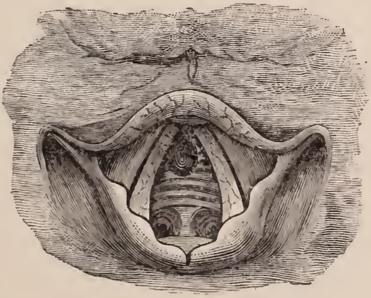

FIG. 2.

Although the laryngoscopic examination of this patient was exceedingly easy, and his well-proportioned fauces and pharynx tolerated the use of my largest-sized mirror (having a diameter of 3½ cm.), it was far from easy to ascertain that the tumor was temporarily indentable though of rather firm consistence. The introduction of instruments into the larynx was very badly borne; nevertheless, the diagnosis of angioma was made from the appearance of the tumor at the first visit to the Polyclinic.

The catarrhal condition was ameliorated by means of cleansing and alterative applications of my ethereal iodoform solution, and I had to go through a regular system of training for the purpose of gaining access to the tumor. Its precise attachment, and the circumstances of the case, made its proper removal—especially in a patient who depended for his future livelihood upon his singing voice—one of the most delicate and difficult of operations.

On Nov. 23d, I attempted its evulsion with Cusco's forceps, of Robert & Collin's make, but on account of the peculiar bend of that instrument with its antero-posterior opening, I succeeded in pinching off only a small portion of the posterior and lateral end of the tumor. Some bleeding was the result at the time, and some sloughing and diminution of the size of the tumor followed after. On Nov. 30th, I used my modification of the Cusco forceps,—*i. e.*, one with a bend of one and a quarter right angle,—seized the whole of the tumor close to the edge of the vocal band and its anterior insertion, compressed the tumor forcibly, and squeezed it off—bringing it out in the hollow of the forceps. Less bleeding took place than when I pinched off the small portion, and a slight cauterization with nitrate of silver completely arrested it very soon. I insisted upon the patient's absolute vocal rest, and for several weeks he wrote all he wished to communicate. I also attended to his general health and regimen, and kept him from all physical and emotional excitement. For some days there was a good deal of suggillation seen in the right vocal band, and hyperæmia and congestion of the part remained for more than three weeks. But he has recovered completely, and no vestige of the tumor can be detected on the closest examination.

At the meeting of the New York Pathological Society, December 12, 1883, I presented this tumor,[1] microscopical examination of which confirmed the diagnosis of angioma.

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[1] See Transactions of New York Path. Soc., *New York Medical Journal*, January 12, 1884, p. 51; *N. Y. Med. Record*, January 5, 1884, p. 21; *Philadelphia Medical Times*, December 29, 1883, p. 264; etc.

A. DEFINITION AND SYNONYMS.

Angioma is a tumor consisting of a blood-vessel growth. It is not simply a blood tumor or hæmatoma, and differs from a varix or an aneurism, *i. e.*, a dilatation of either venous or arterial blood-vessels on the one hand, and a vascular cyst on the other. According to Virchow[1] no growth ought to be regarded as an angioma which does not essentially consist of newly formed blood-vessels, or of blood-vessels with newly formed elements in their wall. Heitzmann says: "The characteristic feature of angioma is an abundant supply of blood-vessels—arterial, venous, or capillary. This causes its erectibility, *i. e.*, its swelling on a spontaneous engorgement of the vessels."[2]

The name "Angioma" is preferable to "Angionoma," which latter is the term used by J. Hughes Bennett,[3] and Follin,[4] but which, as Virchow has pointed out, is etymologically incorrect.

As synonyms the terms "vascular tumor" and "erectile tumor" have been employed, and by different authors also a number of others which have given rise to much confusion.[5]

B. CASES OBSERVED.

I have stated that in addition to my own two cases I know of but three instances of intra-laryngeal angioma. These are the following:

CASE 3.[6]—Dr. Fauvel, of Paris, was consulted on June 4, 1872, by a merchant of Lille, fifty-three years old, in good general health, who, without known cause had become hoarse about a year previously. There was no cough, no feeling of foreign body in the larynx, no pain. He breathed without difficulty; auscultation elicited nothing wrong in his chest; there was no specific history. The laryngoscope showed in the angle of the vocal bands, on the level of their anterior commissure, attached on the right side, a non-pediculated red tumor, blackish in places, about as large as a filbert. After prescribing bromide of potassium for six days, and accustoming the patient to the contact of his forceps, Fauvel removed the tumor by evulsion in five separate sittings. Some bleeding ensued each time, and for some days the bands appeared red and ecchymosed. Microscopical examination proved the tumor to be angiomatous. The patient was cured and no recurrence took place.

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[1] Die krankhaften Geschwülste. Von Rudolf Virchow. Berlin, August Hirschwald, 1867, vol. III., p. 308.

[2] Microscopical Morphology of the Animal Body in Health and Disease. By C. Heitzmann, M.D. New York, J. H. Vail & Co., 1883, p. 512.

[3] Clinical Lectures on the Principles and Practice of Medicine. Edinburgh, 1858, p. 156.

[4] Traité élémentaire de pathologie externe. Paris, 1861, vol. I., p. 204.

[5] Compare Virchow, *op. cit.*, p. 307, *et. seq.*

[6] Traité pratique des maladies du larynx. Par le Dr. Ch. Fauvel. Paris, V. Adrien Delahaye et Co., 1876, p. 545. (Observation 185.)

CASE 4.[1]—On May 1, 1873, a man, thirty-seven years old, came to Dr. Fauvel on account of a severe hoarseness which had lasted about seven months. At times his voice was clear, but suddenly, without appreciable cause, it gave way. Laryngoscopical examination revealed on the free edge of the left vocal band, very near its anterior insertion, a tumor of the size of a pea, blackish red, smooth, slightly pediculated, which prevented the approximation of the vocal bands. The patient had no idea of the presence of the tumor; he had no sensation of a foreign body; he did not cough; breathed without difficulty, and, as he said, if he were not hoarse, would be in perfect health. After the patient had taken some bromide of potassium, ineffectual attempts of removal of the growth were made on May 5th, 8th, 12th, and 15th. On May 19th Dr. Poyet, Fauvel's chief of clinic, pinched off the little tumor with forceps without teeth. The patient felt nothing, and the voice at once became clear and of good quality. In the larynx was seen only a little bleeding point where a moment before the tumor had been.

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[1] *Ibid.*, p. 606. (Observation 240).

The cure was complete. No recurrence occurred. Microscopical examination proved the tumor to be a cavernous angioma.

CASE 5.[1]—In 1878, a railroad official, thirty-eight years old, looking somewhat anæmic and weak, who, with healthy lungs, had long suffered from chronic pharyngeal and laryngeal catarrh and veiled voice, but had no other symptoms and no idea of a tumor in his larynx, had come to consult Dr. Heinze, of Leipzig, merely on account of his catarrh. Aside from his catarrhal condition, Dr. Heinze found at the border of the right ventricular fold, looking out from under it, near the anterior commissure, a tumor of the size of half a lentil, and behind it, separated by a free interspace, another tumor about half this size, both of the color of a black berry, roundish, and uneven. The entrance to the right ventricle appeared about twice as broad as usual and as that of the left side; and the fold, forming its upper boundary, was pushed up like a dome, especially in its upper portion. To the touch with the probe, both tumors appeared soft, with depressable surface. When the parts had been sufficiently educated to allow the ventricular fold to be pulled away a little with the end of the probe, it was found that the growth came from the depth of the ventricle, and that the two black projections seen at the border of the fold were portions of one and the same neoplasm. The anterior projecting portion—the larger—was removed with an ordinary wire loop; an extremely violent hemorrhage, lasting nearly half an hour, ensued from the stump. About a cupful of bright-red blood was lost before the bleeding was arrested by cauterization with silver nitrate; it recommenced in the evening, and was definitely stopped only by another and very energetic cauterization of the whole ventricular fossa. Very soon after, the smaller projection had become invisible, obviously because the main tumor had shrunken; but in the course of three months, a little black elevation could be detected in the same spot where the larger tumor had been. This enlarged slowly but steadily, and in ten months had attained to the size of a pea. Dr. Heinze destroyed the projecting portion of this neoplasm by repeated punctures with a sharp-pointed galvano-caustic instrument, and cauterized the entire fossa with a flat galvano-cautery bent on its side. These operations produced almost no pain, and but insignificant bleeding. No recurrence has taken place.

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[1] "On Angioma of the Larynx," by Dr. O. Heinze, Leipzig.—*Archives of Laryngology*, vol. i., No. 2, p. 135.

To these five cases I might add a sixth—viz., that of an angio-papilloma, which occurred in the practice of Dr. Fauvel, and which that author reports as a "Papillary polyp, with very pronounced vascular dilatations, making it resemble an angioma."[1] A man, twenty-eight years old, consulted Dr. Fauvel, April 1, 1871, having suffered for two years with a progressive hoarseness that made him fear complete loss of voice. His health was perfectly good, but there was seen, at about the middle of the free edge of the right vocal band, a tumor of the size of a pea, with broad attachment, rounded and smooth, which interfered with the approximation of the vocal bands during phonation. Its color was blackish. The patient took bromide of potassium, in large doses, for two months, and on June 6th, Dr. Fauvel succeeded in removing the tumor by evulsion, but the bleeding was more than usually abundant. The part was cauterized with silver nitrate. The vocal bands, at first very red, recovered in a few days, the patient's voice became normal, and no recurrence of the tumor took place.

Dr. Heinze, in his article in the *Archives of Laryngology*, mentions three cases observed respectively by Drs. Johnson, Fournié, and Mackenzie, which, however, are not cases of intra-laryngeal angioma.

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[1] *Op. cit.*, p. 522. (Observation 163.)

The case of Johnson[1] referred to, a man fifty years old, who consulted the Dr. first in October, 1863, on account of hoarseness, which was much increased by continuous speaking even for a few minutes. Feebleness of the voice had then existed for more than a year. When Dr. Johnson first examined the patient, he saw only the injected state of the mucous membrane, but after repeated examinations he found a red and apparently very vascular tumor between the two vocal bands just at the anterior commissure. Repeated applications of solution of iodine by means of a bent brush were made without decided or permanent benefit, and on June 27, 1865, Dr. Johnson attempted to catch the tumor with a wire écraseur. There was an escape of about an ounce of blood, and in the midst of it a shreddy piece of membrane was found. The laryngoscope showed that the tumor was gone ; in its place there was a slightly uneven projection near the anterior extremity of the right vocal band where the tumor had been attached. This spot was occasionally touched with a solution of silver nitrate, and the nodular unevenness quite disappeared. The voice gradually became stronger and clearer. No recurrence took place. Dr. Johnson inferred that the tumor was a vascular cyst containing fluid, that the cyst had been ruptured by the grasp of the wire loop, and that the membrane expectorated had formed a portion of the cyst-wall.

Dr. Fournié[2] has reported two cases of melanotic tumor : One, in a lady, about forty-six years old, suffering from obstinate and severe cough. The tumor was situated in the right vallecula, implanted with a pedicle at the level of the free border of the epiglottis, and overlying the right arytenoid cartilage, and was removed by means of a galvano-cautery platinum loop-knife, which was made to encircle it, and was drawn through the tumor with slight traction. The other was a tumor found in a man about thirty years old, in the angle of the pharynx on the right side extending into the pyriform sinus.

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[1] " A History of Seven Cases of Growth on the Vocal Cords." By George Johnson, M.D. Transactions of the Medico-Chirurgical Society. London, vol. LI., Case 2, p. 176.

[2] " Tumeurs mélanotiques du pharynx " par Dr. Edouard Fournié. *Gazette des hôpitaux*, No. 56, 1868, p. 223.

Mackenzie's case[1] was that of a captain of the Royal Marines, aged thirty-five years, who was sent by Dr. Smyly in December, 1869, complaining of an uneasy tickling sensation in the throat, which had come on in the previous summer, when he had hay-fever. The voice was normal. On laryngoscopical examination, a growth, the size, color, and configuration of a ripe blackberry, was seen in the right pyriform sinus. Several attempts were made at removal, but owing to the extreme hardness of the growth, it was not entirely removed till January 21, 1870. Incomplete evulsion was effected with tube forceps, and the base of the tumor was excised with cutting forceps. The operation was attended with more hemorrhage than is usual, and slight after-bleeding took place for some hours. After its removal, the neoplasm lost its black color, and became pinkish-red. On section its appearance to the naked eye was compact, and its color almost white. On microscopic examination, its structure was seen to be obscurely fibrous, the fibres being very closely intermingled, and the vessels so closely and tightly packed together that they could not be isolated.

In addition to the two cases just mentioned, of Fournié and Mackenzie, I know of two other instances of melanotic or angiomatous tumor in the pyriform sinus, one observed by Fauvel, and one that has occurred in my own practice. It is remarkable that in all these cases the tumor was on the right side. Fauvel's patient[2] consulted him

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[1] Essay on Growths in the Larynx. By Morell Mackenzie, M.D. London, J. and A. Churchill, 1871, Case 89, pp. 188 and 53.

[2] *Op. cit.*, p. 882.

October 15, 1867. He was a man, thirty-five years old, who had for several years been suffering from an irritating, dry cough produced by a tickling sensation in the region of the upper border of the thyroid cartilage on the right side. Otherwise perfectly healthy, he had in his right pyriform sinus a mammillated black growth as large as a bean, which Dr. Fauvel diagnosticated as a melanotic or angiomatous tumor. He destroyed it by three applications, with an interval of five minutes between each, of Voltolini's galvano-cautery. The burning was very bearable and there was no bleeding. The tumor was reduced to a very small escharotic point on the outer posterior laryngeal wall, which remained for a few days and then disappeared. The patient was cured of his cough and morbid sensations, and no recurrence took place.

The subject of my case of angioma of the pyriform sinus was a married lady, fifty-two years old, anæmic and weak, and mother of several children, who consulted me in June, 1873. She had a most annoying, constant inclination to clear her throat, and insisted that she had something growing there. Her voice was unaffected, but she had a peculiar cough, and pain in swallowing. So many physicians had prescribed for her without giving her the least relief that she was greatly discouraged and melancholic. She pointed out the laryngo-pharyngeal region of the right side as the

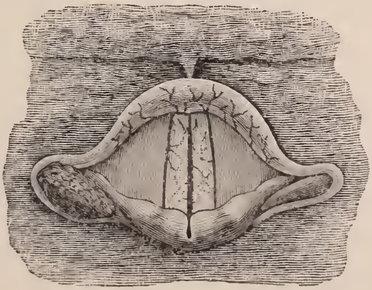

FIG. 3.

seat of all her trouble. She had never been examined with the laryngoscope. The latter revealed a bluish-black rough-looking, flat tumor, as large as the end of my little finger, filling out and stretching the right pyriform sinus (see fig 3).

The tumor was easily touched, but bled on slight provocation. I applied to it daily for a while a diluted solution of persulphate of iron; afterward, every other day, tannin and glycerine; and still later a few times, my ethereal solution of iodoform. On the third of July I removed a portion of it with my ordinary laryngeal forceps. This operation was followed by considerable hemorrhage, which, however, was stopped in fifteen or twenty minutes by the injection of nebulized solution of persulphate of iron. The portion of tumor removed proved to be of angiomatous character, and I destroyed the remaining portion *in situ* by repeated applications of the galvano-cautery. These galvano-caustic operations were usually also followed by some bleeding, but not enough to require recourse to hæmostatic measures. On July 29th the patient left the city cured, and no recurrence had taken place when I saw her five years later.

C.—Pathology and Etiology.

The etiology and pathogenesis of angiomata are by no means clear. Although such growths in other parts of the body are frequently congenital, there is no evidence that any case of laryngeal angioma has existed from birth. On the contrary, on the one hand, the cases observed seem to have clearly had a beginning after adult age had been attained; and, on the other, not a single one of the congenital intra-laryngeal tumors hitherto observed has been angiomatous.

Laryngeal angioma appears to be confined to the mucous membrane and to be accompanied by, or perhaps rather to accompany, chronic catarrhal conditions. The latter circumstance would confirm what seems to be the opinion of Virchow, that a progressive *irritation* [hyperæmia, inflammation, no matter how induced], in the part in which an angiomatous tumor appears, causes granulation which leads to the new formation of blood-vessels, and that some of these becoming isolated, atrophy of intervening tissue is produced. The original distribution of blood-vessels no doubt influences the development of the tumor. It is worthy of remark that in all the cases of intra-laryngeal angioma known, the growth occurred near the anterior commissure of the vocal bands, and in all but one (to be sure there are only five altogether) on the right side. All the cases occurred in the male sex. Heitzmann says we can distinguish three varieties of angioma, according to the nature and distribution of the blood-vessels—simple, lobular, and cavernous: Simple angioma is to a great extent composed of newly formed capillary blood-vessels, between which, in more or less uniform distribution, is a varying amount of fibrous or homogeneous connective tissue; lobular angioma is composed of coils of large capillary blood-vessels, held together by delicate fibrous tissue, while between the coils this tissue is somewhat denser; cavernous angioma is constructed on the plan of cavernous tissue—*i. e.*, composed of venous sinuses, which lie close to each other, separated only by walls of fibrous tissue, in which are capillary blood-vessels and sometimes bundles of smooth muscle-fibres, the cavernous sinuses being filled with blood-corpuscles.

All the five laryngeal angiomata seem to have been of the cavernous variety.

D.—SYMPTOMS AND DIAGNOSIS.

The symptoms of intra-laryngeal angiomata are like those due to other tumors in the same locality, of the same size, form, and consistence. In all the cases observed the voice was affected; thus in Heinze's case it is noted that the voice was veiled; in Fauvel's cases there was much hoarseness—forcing one patient to give up his business; and in my cases there was increasing hoarseness in one and huskiness and acantophonia in the other. This was really the only symptom that came from, and pointed to, the tumor; and I have observed as a striking peculiarity that temporary local congestion from any cause made this symptom more prominent; there was no interference with respiration, no cough, no pain, no difficulty of swallowing.

The diagnosis can be made only by means of a laryngoscopic examination. The tumor is then seen. As I have already said, the seat of the tumor was in each instance near the anterior commissure of the vocal bands; in one case it was within the ventricle, in the others on the free border of the vocal band. The attachment was always what is called sessile; even where there was a pedicle this was short, and, in my case, even lengthwise attached. The shape of angiomatous tumors is usually roundish; the size varying from that of a small pea to that of a bean; the surface either smooth or uneven, the former more often; the coloration reddish- or bluish-black, and the consistence semi-hard. The latter point may be determined by the laryngeal sound or probe; also that the tumor is indentable without being fluctuating. In speaking of the diagnosis of his own case of angioma, Mackenzie says[1]: "No person who saw the growth, before its removal, could doubt its being a true angioma, and in judging of its nature, I rely more on the appearances presented by the living growth, than on the subsequent microscopical examination." I agree with Mackenzie that the correct diagnosis may be made by laryngoscopical examination. There is a liability to make a mistake, however, when the tumor is a hæmatoma, a "vascular cyst" (in which case there is usually fluctuation discernible), or else what might be called a combination angioma, *i. e.*, angio-myxoma, angio-sarcoma, etc. The microscopical diagnosis of the tumor after removal is histologically confirmatory.

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[1] *Op. cit.*, p. 53.

E.—PROGNOSIS.

Although the number of intra-laryngeal angiomata observed is too small to generalize from, the prognosis may be said to be, with proper treatment, generally favorable both as to life and as to voice. But, in making a prognosis, the fact must not be overlooked, that owing to their situation near the anterior commissure of the vocal bands, the treatment of such tumors belongs to the most difficult operations in laryngo-surgery. They have, however, not been observed to be large enough to cause death by suffocation, and the voice seems to have been entirely restored in each case. The danger from hemorrhage after operations will be spoken of under the heading of treatment, and, of course, special circumstances can render the prognosis, as is the case with other tumors in the interior of the larynx, unfavorable. Recurrences have not been observed to take place.

F.—TREATMENT.

The only effective treatment is the removal of the tumor. In cases in which the voice alone is affected, there is no indication for any external cutting operation in order to produce artificial access of air, and the tumor should, if possible, be removed by the aid of the laryngoscope through the mouth and the natural upper orifice of the larynx. Thyrotomy is entirely out of the question, and the three other cutting operations for gaining access to an intra-laryngeal tumor, viz., subhyoidean pharyngotomy, inter-thyro-cricoid laryngotomy, and tracheotomy, are so exceedingly rarely necessary in the cases in question, that I can confine myself to intra-laryngeal methods. These consist in chemical and mechanical means. The purely chemical embrace caustics and escharotics; the purely mechanical embrace cutting, pinching, tearing, and ligating, while galvano-cautery partakes of the character of both means.

As it is known that an operation upon an angiomatous tumor in any situation may be attended with considerable hemorrhage, it is natural to prefer a method which at the same time that it extirpates the tumor is hæmostatic. On this account Heinze recommends galvano-cautery above all other means. He says: "As to treatment, it may be laid down as the rule, that in order to avoid possible danger, since it cannot beforehand be known whether a tumor of this kind will give rise to severe bleeding or not, the galvano-cautery should be employed for the removal or destruction of laryngeal angiomata."[1] The manner in which he carried out this method was by repeated punctures of the tumor with a sharp-pointed galvano-caustic instrument and cauterization of the entire ventricle (*i. e.*, the place of attachment) with a flat galvano-cautery bent on its side. He had previously removed a portion with an ordinary wire loop; and certainly for ligation the heated galvanic wire cannot be used as safely or as frequently as the simple wire of the écraseur, on account of the injury it might inflict upon the vocal organs on account of the difficulty of limiting the action of the heated platinum loop to just the small point that it touches.

On the subject of caustics and escharotics, I agree with Mackenzie, "that if sufficiently powerful to be effective, they are very likely to cause spasm of the glottis, or to give rise to inflammation of the adjacent mucous membrane."[2] On the other hand, if not sufficiently powerful, they are more apt to cause an increase than a diminution of the tumor. When they are to be tried, I would recommend the *careful use* of nitric acid, acid nitrate of mercury, chloride of zinc, or caustic potash, each of which I have successfully employed by means of my applicator in the interior of the larynx in other cases.

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[1] *Loc. cit.*, p. 136.

[2] *Op. cit.*, p. 82.

Taking it all in all, I think that instrumental removal by "pinching," combined with torsion and evulsion, is the most effective, advantageous, and safe mode of operating upon intra-laryngeal angiomata. This seems to have been the manner in which Fauvel (and Poyet) successfully operated in his two cases, and was the means by which I cured my patients. The best instruments for the purpose are my blunt evulsion forceps. I have some made to open laterally, and some to open forward and backward. The bend which I have given them is the same as that of my applicator, viz., one and a quarter right angles, or, more accurately, 114°. The preparation of the patient and the detailed method of procedure of operating upon intra-laryngeal tumors I have first described nearly twenty years ago.[1] Hemorrhage can be controlled by either the inhalation or injection (or both combined) of a nebulized diluted solution of the persulphate of iron.

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[1] *See* Laryngoscopal Surgery, Illustrated in the Treatment of Morbid Growths within the Larynx. Being the Prize Essay to which the American Medical Association awarded the Gold Medal for 1865, p. 25 *et seq.*

TWO PECULIAR CASES OF TYPHOID FEVER—ONE AT THE AGE OF SIX MONTHS; ONE BEGINNING WITH PNEUMONIA, AND WITH HEART FAILURE CONSPICUOUS.—EFFECT OF DIGITALIS.

By MARY PUTNAM JACOBI, M.D.

CASE I.—*Typhoid in infant six months old. Pneumonia at outset. Recovery.*

The following case occurred in the practice of my friend, Dr. A. S. Daniels.

The most striking peculiarity of this case is the tender age of the patient—six months. On account of the rarity of typhoid fever at this age, the diagnosis was at first much embarrassed; and the embarrassment was increased by the early onset of cerebral and pulmonary symptoms. The illness lasted from the 21st of July until the middle of August, the first complete defervescence being observed on the 11th. The duration was thus three weeks. Diarrhœa existed for two or three days without fever. Then, on July 21st, the child had a slight convulsion and chill (?) (according to statement of mother), followed by fever, which persisted till the 24th, when the temperature was found to be 105°, and the pulse 140. At this time the diarrhœa had ceased, but the child exhibited a combination of cerebral and pulmonary symptoms, such as is not infrequently seen in the pneumonia of children.

Pulmonary Symptoms.

The respirations were eighty-eight in number, and the breathing harsh all over both lungs, while tubular breathing existed at the apex of the right.

Cerebral Symptoms.

The pupils were somewhat contracted; the conjectiva injected, and muscles of the neck somewhat rigid, causing some retraction of the head.

The slightest movement caused the child to cry violently ; but when undisturbed, she remained quiet.

A diagnosis was made of pneumonia, with the hyperæmia of the basal meninges, which so often accompanies pneumonia of the apex in young children. The next morning the temperature had risen to 105¼°. There had been no stool for three days ; a dose of rhubarb and magnesia was given, and in the evening the temperature had fallen to 104°, and from this date never rose above 104.5°, indeed only reaching that point once.

The temperature is shown on the accompanying chart (A.), and may be also tabulated as follows :—

July 24th,	Evening,	105°.
“ 25th,	A.M.	105¼°.
“ “	P.M.	104°.
“ 26th,	A.M.	103¾°.
“ “	P.M.	103°.
“ 27th,	A.M.	104°.
“ “	P.M.	103°.
“ 28th,	A.M.	103°.
“ “	P.M.	104.5°.
“ 29th,	A.M.	103¼°.
“ “	P.M.	101°.
“ 30th,	A.M.	102°.
“ “	P.M.	102¾°.
“ 31st,	A.M.	102°.
“ “	P.M.	102°.
Aug. 1st,	A.M.	102°.
“ “	P.M.	103°.
Aug. 2d,	A.M.	102°.
“ 3d,	A.M.	102°.
“ “	P.M.	103°.
“ 4th,	A.M.	101¾°.
“ “	P.M.	102°.
“ 5th,	A.M.	101°.
“ “	P.M.	103¼°.
“ 6th,	A.M.	102°.
“ “	P.M.	103°.
“ 7th,	A.M.	101¼°.
“ “	P.M.	101¼.
“ 8th,	A.M.	103°.
“ “	P.M.	100.5°.
“ 9th,	A.M.	100.5°.
“ “	P.M.	99°.
“ 10th,	A.M.	99°.
“ “	P.M.	100°.

The maximum height of the temperature is reached at the beginning of the period of observation, circumstances different from the typical curve of typhoid. But the child had been already sick with fever for three days when the observation began, so that the possibility of a more gradual ascent of temperature is not excluded. This brusque début is paralleled in the annexed temperature curve of a typhoid case described by Cadet de Gassicourt[1] (chart B.).

The first descent, from 105¼° to 104°, took place in the evening, but after the operation of a purgative. The subse-

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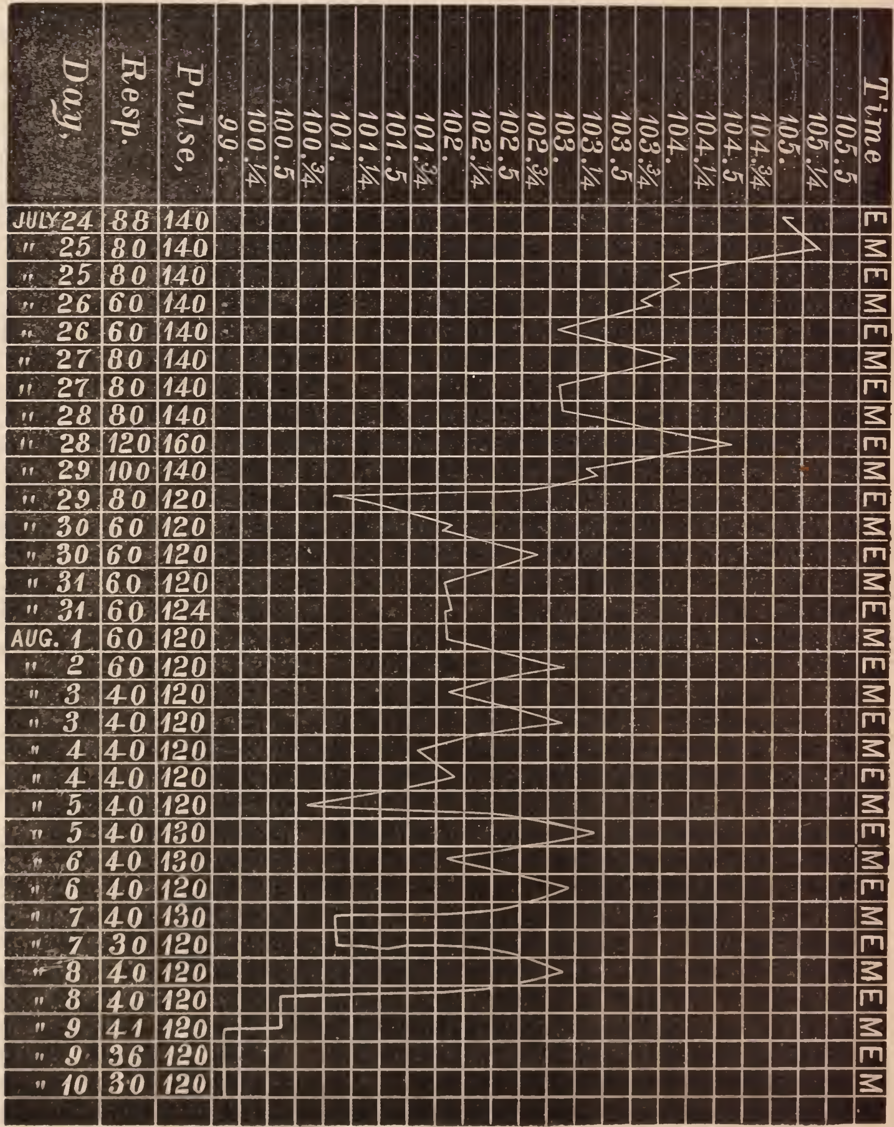
[1] “Maladies des enfants.” Paris, 1880.



Day.	Resp.	Pulse,	Time
JULY 24	88	140	E
" 25	80	140	M
" 25	80	140	E
" 26	60	140	M
" 26	60	140	E
" 27	80	140	M
" 27	80	140	E
" 28	80	140	M
" 28	120	160	E
" 29	100	140	M
" 29	80	120	E
" 30	60	120	M
" 30	60	120	E
" 31	60	120	M
" 31	60	124	E
AUG. 1	60	120	M
" 2	60	120	E
" 3	40	120	M
" 3	40	120	E
" 4	40	120	M
" 4	40	120	E
" 5	40	120	M
" 5	40	130	E
" 6	40	130	M
" 6	40	120	E
" 7	40	130	M
" 7	30	120	E
" 8	40	120	M
" 8	40	120	E
" 9	41	120	M
" 9	36	120	E
" 10	30	120	M

CHART A.

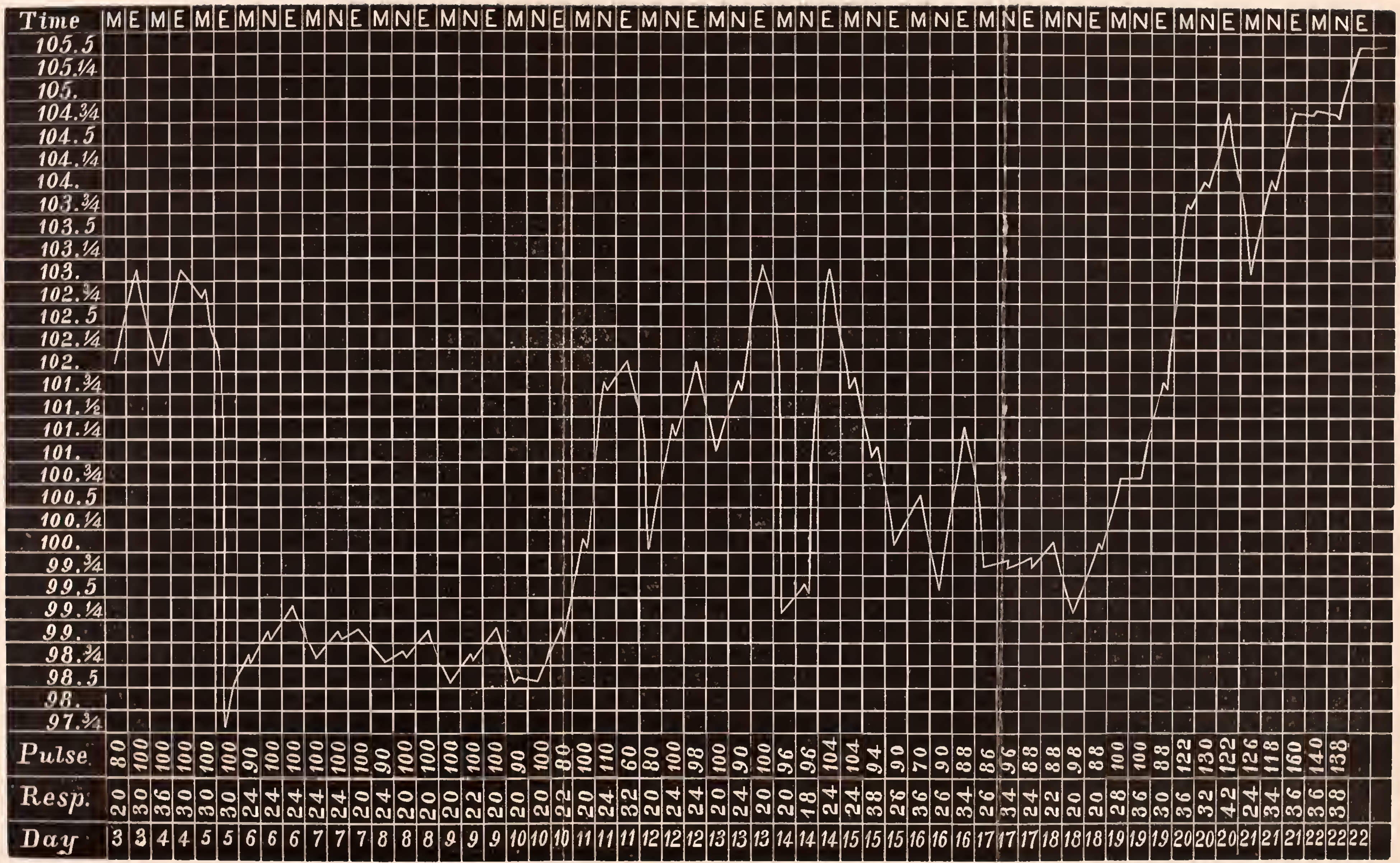

Time
M E M E M E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E M N E
105.5
105.1/4
105.
104.3/4
104.5
104.1/4
104.
103.3/4
103.5
103.1/4
103.
102.3/4
102.5
102.1/4
102.
101.3/4
101.½
101.1/4
101.
100.3/4
100.5
100.1/4
100.
99.3/4
99.5
99.1/4
99.
98.3/4
98.5
98.
97.3/4
Pulse
80 100 100 100 100 100 90 100 100 100 100 100 90 100 100 100 100 100 90 100 80 100 110 60 80 100 98 100 90 100 96 96 104 104 94 90 70 90 88 86 96 88 88 98 88 100 100 88 122 130 122 126 118 160 140 138
Resp.
20 30 36 30 30 30 24 24 24 24 24 20 24 20 20 20 22 20 20 20 22 20 24 32 20 24 24 20 24 20 20 18 24 24 38 26 36 26 34 26 34 24 22 20 20 28 36 30 36 32 42 24 34 36 36 38
Day
3 3 4 4 5 5 6 6 6 7 7 7 8 8 8 9 9 9 10 10 10 11 11 11 12 12 12 13 13 13 14 14 14 15 15 15 16 16 16 17 17 17 18 18 18 19 19 19 20 20 20 21 21 21 22 22 22

CHART C.

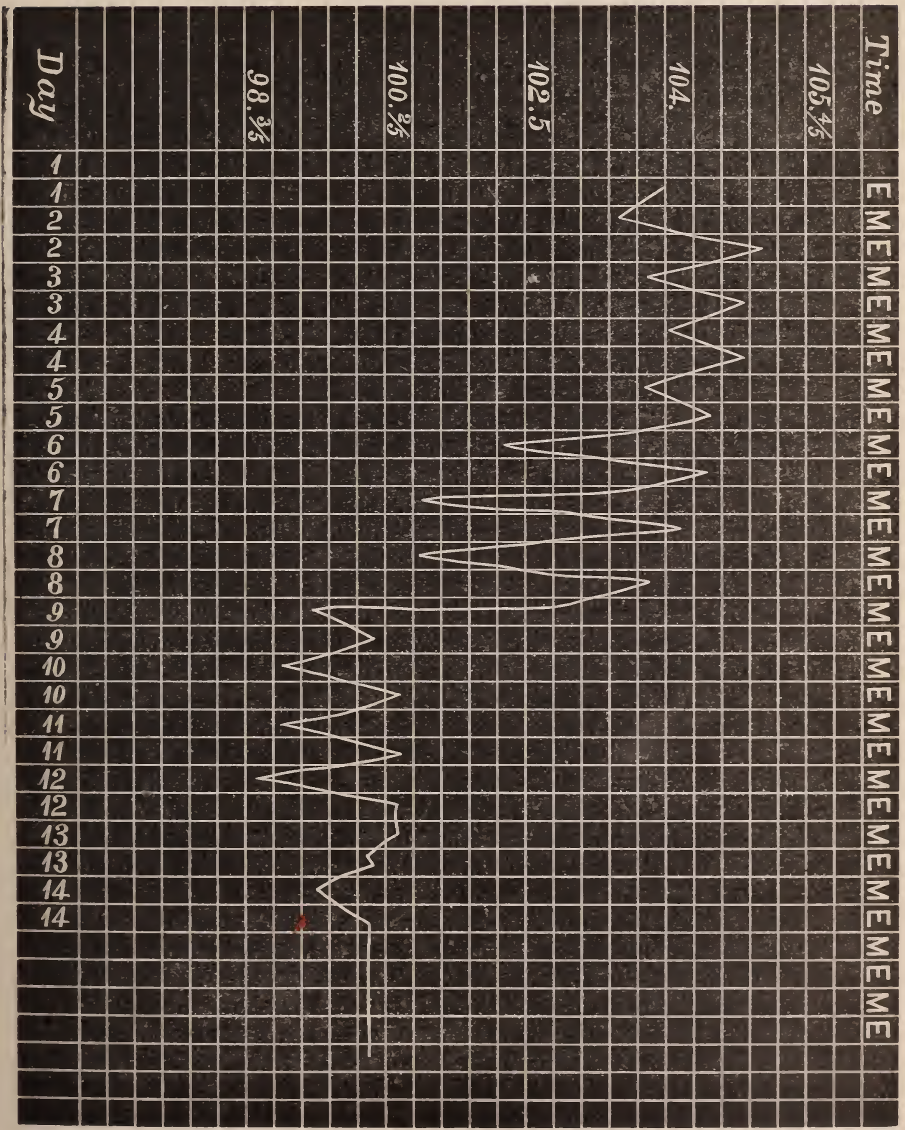

Day
98.3/5
100.2/5
102.5
104.
105.4/5
Time
1 1 2 2 3 3 4 4 5 5 6 6 7 7 8 8 9 9 10 10 11 11 12 12 13 13 14 14
E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E

CHART B.

quent oscillations are irregular, the maximum of temperature occurring in the morning, not only on the fifth, but on the sixth, seventh, and ninth days. On the tenth day, however, the temperature rose in the evening nearly to 103°, from a morning temperature of 102° ; and there was a similar evening rise on the twelfth, thirteenth, fourteenth, fifteenth, and sixteenth days of the illness. Three times before the final defervescence the minimum temperature reached 101° or a little below. On the ninth day of the illness (July 29th) the child received grs. v quinine in a suppository ; and the fall of temperature in the evening to 101° may possibly be attributed to that. On the fifteenth day (August 5th) the morning temperature was 100¾°, and no quinine had been taken the preceding evening. The third fall of temperature, which persisted both morning and evening of the seventeenth day, was also spontaneous ; then there was one more rise of temperature to 103°. Quinine was then given by the mouth, grs. iiss ; this was retained, and repeated in six hours. The evening temperature was then 100.5°, and on the following morning the defervescence, which had occurred in lysis, was complete, and maintained.

The sudden high temperature at the outset was evidently associated with the pneumonia which was observed at the very beginning of the symptoms. The classical moment for pneumonia to appear in the course of a typhoid is during the second or third week. But the occurrence of pneumonia in the first week, though exceptional, has been observed sufficiently often to be admitted ; although it necessarily renders the diagnosis for some time doubtful.

Duration of the Pneumonia.

The tubular breathing began to diminish in intensity on the evening of the eighth day (July 28th), or day of the second maximum of temperature (104.5°). On the next day the temperature fell spontaneously, the quinine having been interrupted on account of the vomiting it occasioned ; and on the tenth day (July 30th) the tubular breathing was only heard in occasional whiffs at the apex of the right lung, the breathing elsewhere being nearly normal. Thus the signs of consolidation yielded without being replaced by signs of resolution,—indication that the consolidation had depended upon intense congestion of the lung without exudation. In children, pulmonary congestion frequently occasions a tubular breathing, not to be distinguished from that of a pneumonia with exudation except by the course of the disease. By the twelfth day all morbid signs had disappeared from the lungs; the respirations, originally 80, had fallen to 40, yet the fever continued with rather regular oscillations, the exacerbation being always in the evening.

Abdominal Symptoms.

Simultaneously with the subsidence of the pulmonary symptoms appeared an eruption of rose-colored spots, which confirmed the diagnosis of typhoid fever, already suggested by the severe gastro-intestinal symptoms which had existed from the beginning of the illness. This had indeed been ushered in by a choleriform attack of vomiting and purging, succeeded, during the first days of the fever, by constipation. On the fifth day (July 25th) the abdomen was distended and tympanitic, and the child screamed when it was percussed. On the next day the diarrhœa returned, and the child began to vomit every thing taken into the stomach; the tongue was red, not coated; the stools thin, yellow, bad smelling. On the tenth day marked tenderness was discovered in the right iliac fossa, and on this day half a dozen rose-colored spots were discovered on the chest. These disappeared on pressure, to return rapidly as pressure was removed. On this same day the temperature began its third distinct rise.

On the eleventh day (Aug. 1st) this eruption began to fade; three or four spots appeared on the abdomen, which remained two days, then faded, to be replaced by a few others. The diarrhœa and vomiting were finally controlled by opium and bismuth, while quinine was suspended, and, after August 9th (the nineteenth day), did not return.

Splenic dulness was found increased on the eighth day (July 28th).

Course of the Cerebral Symptoms.

The rigidity of the muscles of the neck, with consequent retraction of the head, noticed on the first day of observation, persisted until the 28th (eighth day of illness, fourth of observation), then began to lessen, but did not entirely disappear until August 5th (fifteenth day). On the sixth day the child had a slight convulsion, after which she "lay as if dead," according to the mother's account.

The sparsity of the eruption, the early onset of the pneumonia, the degree of irregularity of the temperature curve, the brevity of the disease, may, of course, suggest the suspicion that the foregoing case was one of pneumonia, with reflex hyperæmia of the cerebral meninges, and a complication of gastro-intestinal catarrh. The diagnosis of typhoid fever is based on:

The coincidence of cerebral, pulmonary, and gastro-intestinal symptoms.

The persistence of the fever after the subsidence of the first two sets of symptoms while the last continued.

The diurnal oscillations of the fever, becoming more regular as the tubular breathing disappeared.

The absence of any signs of resolution, indicating that the tubular breathing had depended exclusively on congestion.

The character of the stools,—thin, yellow, ochrey,—not green, as in ordinary gastro-intestinal catarrh.

The tympanites and abdominal pain, with maximum of severity in right iliac fossa.

The appearance of the eruption on the tenth day, together with enlargement of the spleen.

CASE 2.—*Typhoid fever with sudden beginning. Heart failure. Effect of digitalis.*

The patient was a woman aged forty-five, who, during some years, had been subjected to overwork and severe mental strain.

Début of the Disease.

The onset of the disease was sudden, beginning with a severe chill. This was followed during the night by fever and a pain in the left hypochondrium. The fever persisted continuously during two days. On the third she was seen for the first time by a physician. During these first two days the patient was occasionally out of her mind, but not actively delirious.

When first visited by the attending physician, it was noticed that the intelligence of the patient was already clouded. The facial expression was apathetic; the patient only spoke in answer to questions, and although the replies were intelligent, the patient was semi-stupid in the intervals of speaking. She complained of nothing. The pupils were somewhat contracted. The temperature under the tongue was 102° (in the morning); the pulse only 80, compressible, weak, at times irregular. The heart-sounds were remarkably feeble, but there was no valvular murmur. The respirations were 30. Over both sides of the chest the respiratory murmur was extremely harsh, more markedly so on the left side. There were no râles, however, and percussion resonance was normal. There were no abdominal symptoms; the urine had a sp. gr. of 1030, excess of phosphates, no albumen.

First Diagnosis.

The suddenness of the début, the severe chill, hypochondriac pains, harsh and accelerated respiration, indicated very plausibly a central pneumonia with peri-pneumonic congestion. The feeble heart and clouded intelligence, at so early a period of the disease, was sufficient evidence of an adynamic type of disease.

During the first two days of the fever, the patient had been taking repeatedly homœopathic tincture of aconite. The question suggested itself whether this medicine had not been really stronger than supposed, and had thus caused the cardiac feebleness noticed on the third day.

Stimulation of the heart was evidently required. The patient, on moral grounds, absolutely refused to take brandy. The attending physician ordered digitalis in doses of five drops of the tincture every two hours, as a "cardiac tonic." Ten grains of quinine was also ordered in two doses, and a diet of milk and eggs.

Effect of Treatment.

After four doses of digitalis had been taken, the pulse, in the evening, rose to 100, and became stronger; the temperature rose to 103°. The patient was more stupid, lay with her eyes closed, and frequently muttering. Insomnia and delirium succeeded in the night. Ten grains of quinine were again given, and the following morning the temperature had fallen to 102°; the pulse remained at 100, but fairly strong; the respirations had risen to 36. On the evening of this, the fourth day, the temperature again rose to 103°. The pulmonary symptoms had not progressed.

Second Diagnosis.

A probable diagnosis of typhoid fever was made, with determination of morbid process to the lungs preceding symptoms of intestinal lesion.

First Collapse.

At 7 P.M. of the fifth day, the patient, after two or three hours' restlessness, passed into a state of moderate collapse. She became quite unconscious; the face pale, extremities cold. The temperature fell to 97¾°, the pulse remained at 100, but became very weak and irregular. The first sound of the heart was feeble. The respirations fell to 30; respiratory murmur continued to be harsh over both lungs, while numerous liquid subcrepitant râles were heard. Slight blowing respirations at the upper third of left lung.

Nature of Collapse.

"Collapse," observed Wunderlich, "is a generalized disturbance whose immediate anatomical cause is at present unknown, but which, as a morbid process, may be described in the same way as fever. It is more frequent in abdominal affections than any other."[1] "The direct cause of collapse is any that directly depresses the force of the heart, that is of ventricular contraction."[2]

Explanations of Collapse.

At the date of the disease on which I first saw the patient,—the fifth day,—there had been no diarrhœa, and even no abdominal pain indicating intestinal lesion. There seemed, therefore, no reason for supposing the existence of an unusually extensive typhic infiltration as cause of this early collapse.[3] A diagnosis of typhoid fever seemed probable, on account of the regular oscillations of temperature, the cerebral symptoms, and the obscurity of the pulmonary symptoms. Nevertheless, this diagnosis could not yet be said to be positive, and the entire cortege of symptoms agreed with the original hypothesis of a central pneumonia of adynamic type, with peri-pneumonic congestion and bronchitis. On this hypothesis the phenomena of collapse might depend on pulmonary œdema, whose physical sign was the numerous fine liquid râles, heard all over the chest. The respiration, however, was not that of pulmonary œdema; it was neither superficial nor frequent enough.

By a third explanation this early collapse would be attributed to the digitalis, which had been taken in the doses mentioned during forty-eight hours; five drops of the tincture having been taken every two hours. The patient had received ʒ i a day, or ʒ ii in all, in much divided doses. In ʒ i of the tincture there are 7.44 grains of powdered digitalis.[1] Thus the total amount taken in forty-eight hours was fifteen grains. According to the method of Hirtz, 0.75 or 1.00 of the powder is administered in infusion during a single day, and the administrations continued during four days. This gives from 13 to 13½ grains a day, and a total of 52 to 62 grains in all.[2] The quantity taken by Mrs. A., as compared with this, seemed altogether too small to be responsible for the collapse symptoms.

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[1] *Archiv der Heilkunde*, 1861. "Der Collaps in fieberhaften Krankheiten."

[2] Ackerman: "Beitrage zur Theorie des Collaps," *Virchow's Arch.*, Bd. xxv.

[3] Griesinger (Die Infections Krankheiten) quotes from Dittrich and Hamernyk (*Prager Vierteljahrs.*, 1846, Bd. x) cases of generalized infiltration of all the glands of the intestine, from the duodenum to the rectum. Death ensued with choleriform symptoms.

Another fact seemed to tell against the influence of digitalis in producing the collapse. In digitalis-collapse the pulse becomes slow, while in collapse during fevers from causes associated with the morbid process, the pulse is usually very rapid. A notable feature in the change of pulse which occurred after the first doses of digitalis was, that its frequency, instead of diminishing, increased as it grew stronger, without, however, becoming excessive ; the number of beats was from 80 to 100.

When the collapse set in the pulse remained at 100, but became weak and irregular.

The digitalis was suspended for six hours, and liberal stimulation administered by means of brandy, ether, and camphor. By the following morning (and after the resumption of the digitalis), the patient was much better. Consciousness was restored ; the temperature had risen to 98¾° ; the pulse, still 100, had become regular and fairly strong ; the respirations, 24. The respiratory murmur was still harsh, but the rales had greatly diminished in number. Blowing respiration was heard over left scapula. As a remedy for the pulmonary congestion present during the collapse, as either cause or consequence (of the heart failure), pilocarpine had been given hypodermically on the preceding evening. It had, however, produced no effect. On this, the morning of the sixth day, a new specimen of pilocarpine was procured from a different druggist, and ⅓ grain given hypodermically. This was followed by sweating which lasted profusely for two hours. The patient also vomited once or twice. The patient then received brandy, ℥ ss an hour in four doses, together with a potion containing carbonate of ammonia, camphor, and compound spirits of ether. The patient received an abundant amount of milk and Leube's meat solution. In the evening she seemed much better than at any time during the illness. Temperature 99.5°; pulse 100, strong, and heart-sounds much stronger; respiration 24.

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[1] The tincture is made with 4 oz. to 2 pints or 32 oz. of alcohol,—thus one part to eight. A drachm, therefore, contains 7½ grains ; and five minims, .62 of a grain.

[2] "Nouveau dictionnaire de médecine : art. digitale."

Second Period.

During the next four days, the temperature, though oscillating upward in the evening, remained within such narrow limits that the maximum was 99¼°, the minimum 98.5°. The morbid sounds in the lungs, instead of increasing, diminished, and the patient seemed to be merely suffering from the prostration consequent on a severe acute illness.

With this apyrexia, and in absence of symptoms pointing to any organic lesion or definite morbid process, it was difficult to avoid believing that convalescence from a limited central pneumonia had been ushered in by the collapse of the fifth day. Two circumstances, however, were inimical to this belief: the pulse remained constantly at 100, though the respirations were between 20 and 24; and the intellect was constantly clouded. Thus there was a strong probability that the apyrexia indicated no real convalescence, but an artificial or incidental depression of temperature, while the morbid process with which fever should naturally be associated, was continuing to progress.

Third Period.

On the ninth day, and for the first time, diarrhœa set in and with great severity, greatly increasing the weakness of the patient. On the eighth day, the abdomen had become slightly tympanitic, but there was no pain in the iliac region, and no enlargement of the spleen. The temperature remained depressed. Seven stools occurred in one afternoon, of which five in the course of three hours. At 6 P. M. the patient was found very weak; the pulse feeble—varying from 80–100; first beat of heart scarcely audible. Face pale, and also the extremities cold. The digitalis had been interrupted for six hours. Bismuth and pepsine were given for the diarrhœa.

Convallaria.

At 9 P. M., the partial collapse continuing, the pulse remaining very weak, and frequently irregular,—it was decided to try the effect of convallaria; 10 minims of the fluid extract were given at 9, and repeated at 11. Brandy was continued. At 1 A. M. of the tenth day, the pulse was weaker, intermittent, 80; general appearance of patient much worse. Eyes closed, face and extremities cold.

Return to Digitalis.

The digitalis, which had now been interrupted during twelve hours, was resumed in five-drop doses every two hours; after that, pulse rose from 80–100, became regular and stronger, and continued to improve, until by 6 A. M., it regained the status previous to the collapse. During the tenth day, the diarrhœa was checked, but returned with violence on the eleventh. At noon of this day the temperature rose, for the first time since the fifth day, to 101¾°, and in the evening to 102°, with a pulse of 110. In the lungs could be heard blowing expiration at upper third of right lung; at apex of left scapula, an occasional whiff of tubular breathing; and at the base of the left lung, distinct tubular breathing and occasional rather fine subcrepitant rales. During this day opium was added to the bismuth powders, with directions to be taken until the diarrhœa was checked. By an error of the attendant, the opium was given in larger quantities than intended,—so that two grains of the powder were taken in about four hours. The diarrhœa ceased, but during the night a third attack of collapse occurred. After recovery from this, the patient became, for the first time, actively delirious, then semi-comatose and cyanosed. By the gestures and cries of the patient just before she passed into the latter state, she seemed to be suffering from severe abdominal pains. These proved to be due to distention of the bladder by an enormous quantity of urine (36 oz.). When this had been drawn off, the patient became at once tranquil; the cyanotic tint disappeared from the face, and the pulse became stronger. The urine had no albumen, but excess of urates, and diminution of chlorides.

Typhoid Eruption on Twelfth Day.

On this, the twelfth day, an eruption of roseola was discovered over the abdomen, extremely abundant and characteristic.

During the next three days I did not see the patient. The attending physician continued the same stimulating treatment, with moderate amount of opium, holding the diarrhœa in check. On the morning of the 13th day the digitalis was suspended, and continued suspended during 4½ days, thus till the afternoon of the 17th. During this period the attacks of collapse continued to recur more and more frequently. The range of pulse, temperature, and respiration was as follows. [See also chart C.]

13th day	Temp.	A.M. 101°	Noon 101¾°	P.M. 103°
	Pulse	“ 98	“ 100	“ 90
	Resp.	“ 20	“ 24	“ 24
14th day	Temp.	A.M. 101½°	Noon 99.5°	P.M. 103°
	Pulse	“ 66	“ 96	“ 104
	Resp.	“ —	“ —	“ 24
15th day	Temp.	A.M. 101.3°	Noon 101°	P.M. 100¾°
	Pulse	“ 104	“ 94	“ 90
	Resp.	“ 26	“ 38	“ 26
16th day	Temp.	A.M. 99¾°	Noon —	P.M. 101¼°
	Pulse	“ 96	“ —	“ 88
	Resp.	“ 34	“ —	“ 34
17th day	Temp.	A.M. 99¾°	Noon —	P.M. 100°
	Pulse	“ 86	“ —	“ 60–50
	Resp.	“ 26	“ —	“ 20 labored.

On this last day the fall of the pulse in the evening had been preceded by two doses of digitalis taken during the afternoon.

The resumption of the digitalis was dictated by two observations: 1st, the improvement of the pulse that followed its administration on the third and also on the ninth day; 2d, the continuance and even increase of the symptoms of heart failure during the four days and a half that the digitalis was discontinued, and during which liberal stimulation was employed. The diarrhœa was quite moderate. The four non-digitalis days belong to two different temperature periods. On the 13th and 14th the evening temperature was 103°. But the 15th and 16th belong to a second period of antipyrexia. This was ushered in on the evening of the 15th day by an alarming collapse occurring in the afternoon. The entire body was bathed with cold perspiration, and, as was now constantly the case in these “sinking attacks,” the face became partially cyanosed. In the evening the temperature was 100¾.° From this date to the 18th inclusive, the temperature ran as follows. [See chart c.]

	15th.	16th.	17th.	18th.
A.M.	101.3°	99¾°	99¾°	100°
P.M.	100.¾°	101¼°	100°	100°

THE RANGE OF PULSE.

	15th.	16th.	17th.	18th.
A.M.	104	96	86	88
P.M.	90	88	60–50	Imperceptible.

On the evening of the eighteenth day, and just at the close of this second low temperature period, the patient, who had had many sinking spells during the day, was at 9 P.M. apparently dying. First sound of the heart inaudible; the face mottled;

the lower jaw dropped; the entire surface of the body covered with a cold perspiration; the eyes open with slightly contracted pupils; breathing stertorous.

Musk.

At this time ten grains of musk were inserted into the throat, and a hypodermic syringeful of brandy given—this at 11;—at 11.30 power of swallowing returned, and patient began to rally; at 12 musk repeated; at 1 A.M. the whole appearance of the patient was changed. She was sleeping quietly; the face pale but not mottled; pulse full but not strong; the patient assisted in turning herself in the bed. At 2 A.M. musk repeated. The patient recognized her son at 6 P.M. The temperature was 100$\frac{3}{4}$°, pulse 100, and stronger than at any time in forty-eight hours; first sound of heart returned. The patient continued in this improved condition until 9:30 P.M., when she again became almost unconscious and refusing to swallow. The pulse fell to 80. During the day, owing to some extraneous circumstances the musk was not repeated, so that the patient only took in all, twenty grains. The nineteenth day ushered in the second and final febrile period. The temperature in the morning was 101°, in the evening, 103°, with a pulse of 110. The range of pulse and temperature during next two days was as follows:

19th	Temp.	A.M. 101°	Noon 101$\frac{3}{4}$°	P.M. 103°
	Pulse	“ 96	“ 100	“ 110
	Resp.	“ 26	“ 34	“ 34
20th	Temp.	A.M. 103.5°	Noon 104°	12 P.M. 104$\frac{3}{4}$°
	Pulse	“ 122	“ 124	“ 122
	Resp.	“ 36	“ 30	“ 42
21st	Temp.	A.M. 103°	Noon 104$\frac{3}{4}$°	P.M. 104.5°
	Pulse	“ 126	“ 122	“ 160
	Resp.	“ 24	“ 30	“ 42
22d	Temp.	P.M. 104$\frac{3}{4}$°	Noon 104$\frac{3}{4}$°	Death at 2.45 P.M.
	Pulse	“ 124	“ 140	
	Resp.	—	“ 36	

During this second febrile period, cold packs were used for the first time on the 20th day. During this day the pulse and temperature varied frequently. The cold pack repeated every four hours on the evening of the 21st day, the temperature having risen by 8 P.M. to 104.5°, and the pulse to 160, the packs were applied every fifteen minutes for a couple of hours. At 10 P.M. temperature had fallen to 103$\frac{3}{4}$°, and pulse to 112.

During the 21st day the cold-water treatment was continued, but by means of cold sponging. Quinine was given with no evident effect. The attacks of collapse continued to recur, but without being accompanied by fall of temperature. In one of these, on the afternoon of the 22d day of the illness, the patient suddenly died, with a temperature of 105.5°, a pulse of 140, and respiration at 38.

The points of special interest in the foregoing case are:

1st.—The suddenness of the début of the disease, and early appearance of pulmonary symptoms.

2d.—The amphibolic temperature curve.

3d.—The remarkable predominance of symptoms of heart failure.

4th.—The effect of the digitalis, and the urgent question whether this were in any way responsible for the cardiac collapse.

1st.—The fact that typhoid *may* begin quite suddenly with a chill, is recognized by Griesinger, as also the difficulties of diagnosis to which this exceptional circumstance is liable to give rise. The appreciable physical signs during the first days of the observation (third and fourth days of the illness) were those of pulmonary engorgement. On the fifth day the râles of typhoid bronchitis appeared, though not at the time distinctly recognized as such. This pulmonary engorgement, when considered in connection with the degree of cardiac feebleness which coincided with it from the beginning, was evidently due to this, and indeed the first important result of the heart failure.

2d.—The temperature curve resembled that which Wunderlich describes as "amphibolic,"—consisting of two febrile periods, separated by a prolonged remission of fever. "This remission may even be ushered in by collapse, and usually begins in the middle of the second or commencement of third week. For several days the symptoms are strikingly favorable; then all at once, there is a relapse. This march of the temperature is often seen in aged persons, or those who have been in ill health previous to the fever."[1]

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[1] Wunderlich: "Medical Thermometry," p. 315.

This double fever curve approximately corresponds to the double period characteristic of all typhoid fever. The first fever period belongs to the characteristic fever process, but may include complications, as pneumonia. Anatomically this period corresponds to the infiltration and commencing ulceration of Peyer's glands. The duration is seldom shorter than fourteen days; never longer than twenty-eight; on an average, seventeen to twenty-one days. The second period corresponds to the regression of the typhoid process, and of the organic alterations caused by it. Anatomically it corresponds to the completed ulceration in the bowel, and also to its cicatrization.

The fever of the first period is typical; that of the second is irregular.[1]

The two periods thus described differ from the amphibolism described by Wunderlich: 1st, in that they are not separated by a febrile remission, as are the two segments of the amphibolic curve; 2d, that they are not peculiar to aged people, or to those in a state of previous ill health; 3d, that the duration of Griesinger's period is rather longer than the first half of Wunderlich's curve.

In Mrs. A.'s case, the first febrile period only lasted five days, then was terminated by the collapse which ushered in the remission of a week's duration. It illustrated the possibility signalized by Griesinger, of pneumonia complication in the first period. But the premature termination of this first febrile period (at five days instead of ten or fourteen) suggests that neither the remission nor the collapse were spontaneous, but the result of artificial intervention.

3d and 4th.—The last two considerations, that of the remarkable predominance of symptoms of heart failure, and the influence of digitalis, may best be taken together.

A certain celebrity attaches to the use of digitalis in typhoid fever, owing to the circumstances of its introduction into practice, and the controversies to which this has given rise.

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[1] Griesinger: Die Infections Krankheiten.

In 1814 Clutterbuck praised digitalis as a specific against fever, and as admirably indicated in typhus.[1]

According to Ferber, Currie compared the effect of digitalis with that of venesection, and Rasori used it extensively in his contra-stimulant system of treatment.

In 1860 Hankel of Leipzig reported eighty cases treated by digitalis,—"45 mit glucklichem ausgang."[2]

In 1862 Wunderlich published a paper in the same *Archiv* to define the indications for digitalis in typhoid fever. He protests against "barbaric statistics" based on entire series of cases, and denounces as a "crude empiricism" the attempt to judge of a remedy by the percentage of cures out of such a series. "A medicine may be entirely improper, though followed by no fatal result, or, on the other hand, excellent, although many fatal cases may occur among those in which it is given" (*loc. cit.*, p. 111).

The next series of observations were made by Ferber and published in 1864 (*loc. cit.*).

In 1865 Thomas published a new series.[3]

Hirtz, of Strasbourg, first introduced into France the systematic treatment of typhoid by digitalis.[4] His method, adapted and somewhat modified from the Germans, has been recently systematically elucidated and defended by his pupil, Bernheim, at Nancy,[5] in a series of clinical lectures that compose an interesting monograph on the subject.

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[1] Dictionnaire en 30 volumes. Quoted by Ferber in *Virch. Arch.*, Bd. xxx., 1864.

[2] *Archiv der Heilkunde*, 1860. It is not clear what the author means by this expression. If that only forty-five out of eighty cases recovered, the success of the treatment could hardly be considered brilliant. But from the context, this does not seem the intention.

[3] *Archiv der Heilkunde*, Bd. vi.

[4] *Bulletin de thérap.*, 1862, and "Nouveau dictionnaire de médecine pratique," art., digitale, 1869.

[5] *Clinique médicale*, 1877.

In Great Britain, Dr. Grimshaw introduced the subject in a communication to a medical society in Dublin, and subsequently reported in the *Dublin Quarterly* for June, 1873. The cases, thirteen in number, in which the digitalis was given, were all of typhus, not typhoid fever. The author based his practice on the strong recommendation of Fothergill of digitalis in "acute asthenic disease," and also on the experiments of "Brunton, Hanfield Jones, Dybkowsky, Pelikan, Hilton, Herzog, Valentin, Stevenson, Traube, Von Bezold, who have definitely established the fact that digitalis increases the force of ventricular contraction."

In the number of the *Practitioner* [1873] which followed the publication of Grimshaw's paper, Anstie commented at length upon it. He declared that the report of Dr. Grimshaw's cases "should dissipate the bugbear of heart depression as a symptom of digitalis treatment." He himself, however, contributed no new cases to the elucidation of the problem.

On the authority of Grimshaw's report, Anstie's comment on it, and a review of Hankel's cases in the *British and Foreign Med.-Chir. Review*, xxvi., 513, Wood declares: "Digitalis is often of great value in various acute diseases, such as adynamic pneumonia and adynamic fevers, by maintaining the heart's action. It can have no effect on the diseases themselves, but may help most opportunely to sustain the heart during a crisis or a period of strain upon it."[1]

Digitalis was originally used in typhoid fever as an antipyretic, and its value measured principally by its effect in reducing temperature. Hankel, however (*loc. cit.*), praised digitalis as a remedy for calming delirium and, indeed, all the nervous symptoms of the disease, but considered an excess of temperature, associated with rapid and weak pulse, to be the main indication for the use of the drug.

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[1] "Treatise on Therapeutics," 1880. 3d edition, p. 145.

Hirtz (*loc. cit.*), in formulating his digitalis method, declares that it is only applicable to the first period of typhoid fever, which, in accord with Griesinger, he recognizes as indicating the primary evolution of the typhic process. Given at this time, digitalis will, in from twenty-four to thirty-six hours, depress the temperature often below the norm. In the majority of cases the temperature begins to rise again in twelve hours, but in a certain number the defervescence is final. Out of fifty-three cases treated with digitalis by Bernheim, terminal defervescence occurred in six.

1st case.—Defervescence third day from first dose, and after total ingestion of 1.50 grammes in infusion (22 grains).

2d case.—Defervescence began on ninth day; complete on seventeenth.

3d case.—Defervescence began on tenth; complete on sixteenth.

4th case.—Defervescence began on thirteenth; complete on seventeenth.

5th case.—Defervescence began on twelfth day after first dose of digitalis; finished the twenty-second.

6th case.—Defervescence began on fourteenth day; finished the twenty-second.

In the forty-seven other observations, the fever returned and continued, notwithstanding the continuance of the digitalis.

As bearing on our case, the influence of digitalis on the temperature is chiefly of importance in estimating how far the defervescence of the fifth day was due to it. The digitalis was originally given by the attending physician on the third day of the illness, with no view to an antipyretic effect, for the temperature of the patient was quite moderate (102° F.). It was directed against the cardiac weakness indicated by a pulse feeble and rather slow for the temperature (80), and also by the marked diminution in the distinctness of the first sound of the heart. For the purpose of a cardiac tonic, the digitalis was given in small doses (five drops every two hours); thus (see note to p. 39), 7.44 grains in twenty-four hours.

The antipyretic method has always employed the infusion of digitalis, either in substance or in powder,—the infusion given in tablespoonful doses every hour. The following amounts have been given:

Hankel.—1.25 grammes in twenty-four hours; continue from three to six days. Total, 3.75 grammes (56 grains) to 7.50 grammes (112 grains).

Traube.—3.75 grains an hour until pulse and temperature lowered.

Thomas.—26 to 30 grains in twenty-four hours.

Ferber.—180 to 210 grains in from twenty-four hours to eight days.

Wunderlich.—10 to 20 grains a day for three to six days (30 or 60 to 60 or 120 grains in all); cease with the decided diminution of pulse.

Grimshaw.—℥ i to ℥ iss. infusion every three hours, thus ℥ viii to ℥ xii in twenty-four hours, containing 53 to 80 grains. This in *typhus* fever.

Hirtz.—5.00 grammes to 10.00 grammes in twenty-four hours (75 to 150 grains) produce defervescence promptly, *and yet are less dangerous than small doses continued for a long time.* Habitual dose: .75, 1.00, or 1.50 during twenty-four hours (11, 15, or 22 grains); this continued until an apyretic effect is begun, then cease. Usually three to four days, or total of 33 to 88 grains.

Bernheim.—Identical with method of Hirtz.

Comparing the doses given in the foregoing clinical experiments with those administered in our case, we should infer, if guided by statistical analogy, that the amount of digitalis received by Mrs. A. previous to the defervescence of the fifth day had been altogether too small to be directly responsible for this. But, individualizing the circumstances as much as possible, we must ask:

1st.—Whether the digitalis in form of tincture would be more depressing than in form of infusion, and thus act more powerfully at a smaller dose and after a shorter time.

2d.—Whether the mode of action of digitalis, as an antipyretic, be such that it is intensified in persons with feeble heart.

3d.—Whether the defervescence observed in this case were not a phenomenon of cardiac collapse, rather than the collapse an element of a rapid defervescence; and whether this collapse were due to the direct action of the infectious agent, or to the direct and unusual action of the digitalis on the heart, apart from any antipyretic influence.[1]

The first question may be promptly answered in the negative. The tincture is much less liable to be depressing than the infusion; on this account it is habitually used in the treatment of apyretic heart diseases, and on this account was selected for Mrs. A.'s case. The method of divided doses employed (five drops every two hours) is identical with that used by the authorities employing the infusion (spoonful from hour to hour).

The second question, however, opens up a wide field of inquiry. By what mechanism does digitalis depress the febrile temperatures? Is this depression secondary to that of the circulation, and is the drug indicated as an antipyretic only when the heart is strong enough to bear this; or is it, on the contrary, particularly indicated when the heart is weak? If the latter be true, is digitalis really indicated as a cardiac tonic in fevers as in apyretic morbid states? Or is the suggestion for such use based on a fallacious analogy, and dangerously misleading, because failing to take into account certain cardiac conditions peculiar to the fever, and not present in apyretic forms of cardiac disease?

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[1] Ackerman: Action of Tartar Emetic, contribution to theory of collapse, *Virchow's Archiv*, Bd. xxv., 1862. "Collapse caused by tartar emetic is due to the directly depressing effect this poison exerts on ventricular contraction." The author infers that such cardiac depression is the primary phenomenon in all collapse.

Wood observes that "there is no physiological basis for the antipyretic theory of digitalis," because it does not depress the temperature below the normal in apyretic states. But the same may be said of quinine and salicylic acid, even of the cold bath,—in fact of all antipyretic agents which act in fever, but either not at all, or only very slightly, in health, or in non-febrile diseases. A drachm of the tincture in twenty-four hours is frequently given in organic heart diseases (ten to twenty drops ter die being the officinal dose[2]), and for a long time, without any change in temperature being observed. The rapidity of the pulse is lowered, the force of ventricular contraction indubitably increased,—yet not the least sign of collapse is observed.

In the cases recorded by the authors we quoted, when a fall of temperature is effected by digitalis, it is always accompanied by fall of pulse, while in some cases the pulse falls, the temperature remaining the same. In forty-two cases observed by Ferber, the pulse was affected first; in the others, simultaneously with the temperature. In ten cases recorded by Wunderlich, the pulse fall was as follows (p. 52):

From Hankel we note the following cases:

Case.—End of second week. Pulse 132. Digitalis given (1.25 grm. a day). On following days the pulse ranged, respectively, 128, 116, 116, 108, then became irregular. Digitalis was stopped, stimulants given, yet the pulse continued to fall, and on the next three days marked 96, 60, 68 beats. It then became irregular.

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[2] The same amount as given in Mrs. A.'s case, but in different distribution.

1st case. (4th day of fever).	30 grains in 36 hours.	Fall from 108 to 52.
2d case. 3d day.	30 " " 24 "	" " 102 " 72.
3d case. 4th day.	45 " " 12 "	" " 120 " 72.
4th case. 3d day.	45 " " 12 "	" " 120 " 68.
5th case. 4th day.	50 " " 24 "	" " 132 " 78.
6th case. 4th day.	50 " " 12 "	" " 108 " 60.
7th case. 3d day.	60 " " 36 "	" " 110 " 50.
8th case. 5th day.	65 " " 24 "	" " 92 " 60.
9th case. 6th day.	80 " " 12 "	" " 113 " 70.
10th case. 5th day.	80 " " 12 "	" " 108 " 70.

In the following case the record of the temperature is also given. Digitalis treatment was begun at the beginning of the fourth week:

	TEMP.	PULSE.	
A.M.	40.8° Cent.	112	———
P.M.	40.8°	104	———
A.M.	40.4°	106	———
P.M.	40.6°	112–132	———
A.M.	40.4°	108	Fierce delirium.
P.M.	40.1°	108	Soft.
A.M.	40.0°	84	Regular.
P.M.	40.0°	100	Cease digitalis.

4.5 grms. taken altogether.

Thomas has sometimes observed a fall of pulse before a fall of temperature. This physician did not consider a moderate frequency of the pulse any contra-indication to the use of digitalis. In seven cases the modification was as follows:

Case 1.—Pulse fell from 78 to 38 in 24 hours.
" 2. " " " 84 " 52 " 12 "
" 5. " " " 112 " 76 " 12 "
" 6. " " " 88 " 48 " 36 "
" 7. " " " 108 " 66 " 12 "

The two following cases detailed by Bernheim (*loc. cit.*)

are interesting for our purpose; for in both, heart failure was conspicuous:

CASE 1.—Third day. Tem. 40° Cent. (104°), P. 140, R. 36. Pulse dicrotic, with feeble tension; diarrhœa.

Ninth day, catarrhal pneumonia right lung; agitation; 38.7° (101.4°), P. 140, R. 38. Then infus. digitalis 0.75 grms. (about 12 grains).

Tenth day, Tem. 39° (102.2°), P. 128, R. 40. Thus pulse fell, temperature rose.

Eleventh day, somnolence; sub-delirium; sweats; fuliginosities on lips and teeth; 38.6° (101.4°), P. 104, R. 48. Continued fall of pulse, and fall of temperature, in 48 hours; rise of respiration.

Twelfth day, tem. 38.9° (102°), P. 80.

Thirteeenth day, tem. 37.8° (100°), P. 64, R. 36. Suppression of digitalis, thus after four potions containing in all forty-eight grms. Tension of pulse increased; less dicrotism.

Effect on both pulse and temperature maintained during three days (13th, 14th, 15th); on the 16th day temperature began to rise, but pulse remained the same, at 70 or 80. Thus the effect on the pulse was produced first, and persisted the longest. By the 20th day the heart's action began to be not only slow but feeble, the prostration and cerebral symptoms increased, and the face began to be cyanosed. Upon this symptom of cardiac failure, the administration of digitalis was resumed in doses of 1.00 (15 grains) per diem.

During two days pulse remained the same.

On third day pulse fell to 64, temperature to 37.2° (98.9°). Suppression of digitalis.

On fourth day pulse irregular, somnolence and delirium persisting. Not until six days after cessation of second dose of digitalis (twenty-eighth day of illness) did pulse rise to 80, with a temperature of 38° (100°), and a marked amelioration was perceptible. Terminal defervescence on thirtieth day.

CASE 2.—Tenth day, tem. 40° (104°) A.M., 40.6° (105.08°) P.M. Pulse 128 A.M., 124 P.M. The first potion of digitalis (0.75) was given on this day, thus a considerable portion between the morning and evening measurements. It failed to affect the temperature, which rose over a degree, but slightly lowered the pulse,—at all events kept it from rising in proportion to the temperature. During the next three days digitalis continued, at same daily dose. On fourth day tem. 38° (100.4°), P. 108. Patient in stupor. Sounds of heart irregular. Digitalis suppressed; stimulants given; but in six hours collapse with vomiting, and fall of pulse to 84, tem. 37.8° (100°). After this the temperature rose moderately during three days, and final defervescence set in on the fifth.

Thus, in the great majority of cases, a fall of temperature caused by digitalis, is accompanied or even preceded by a fall of the pulse rate. The pulse is more readily, more profoundly, and more permanently affected than the temperature.

"The fall of the pulse rate is habitually accompanied by an increase of arterial tension. That is to say, that the work of the heart is increased; that this organ, debilitated by the fever, is re-enforced in its contractility by digitalis, and perhaps opposes more resistance to the febrile combustion; * * * pulmonary hypostasis and œdema are less to be feared." (Bernheim, *loc cit.*, p. 434.)

A fall of temperature occurring in typhoid fever treated by digitalis, though occurring in the first week, should not be attributed to the drug if, at the time, the pulse remain unchanged in frequency. Collapsing defervescence may occur spontaneously during the primary period of typhoid, and is always ominous (Griesinger). In spontaneous collapse, the pulse will be feeble, often irregular, small, thready, extremely frequent, or unusually slow.[1] In digitalis collapse, the pulse is slow, not necessarily below the norm., but much below the febrile rate previously maintained. This is shown in the foregoing illustrations, also in the following fatal case from Ferber, where, as the author observes, "the remedy was certainly not free from responsibility for the unfortunate issue" (*loc. cit.*).

Case six of second series.—Girl of sixteen; received infusion of digitalis, ʒ i. to ℥ vi. water, on eighth, tenth, and twelfth days of illness. In the three days took 210 grains.

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[1] Der Collaps in fieberhaften Krankheiten. Wunderlich.—*Archiv der Heilkunde*, 1861.

On sixth day (before digitalis).	Evening.	.39.2° cent.	P. 108	R. 40
7th.	Morning.	.39.7°	" 112	" 38
8th.	Morning.	.39.25°	" 114	" 32
	Evening.	.40°	" 108	" 32
9th.	Morning.	.39°	" 100	" 36
	Evening.	.40°	" 198	" 28
10th.	Morning.	.38.7°	" 66	" 36
	Evening.	.40°	" 88	" 24
11th.	Morning.	.38.5°	" 64	" 26
12th.	Evening.	.38.25°	" 88	" 24
13th.	Morning.	.37.5°	*Pulseless ;* cold extremities ; different cardiac contractions separated by long intervals ; neither vomiting nor sweating.	*cyanosis.*

13th., early morning, sharp chill and sudden death.

In this case the pulse fell with the temperature, but for neither was the fall more marked than is habitual in the most favorable cases; and this notwithstanding the enormous dose of digitalis administered.[1]

Usually, therefore, digitalis may be expected to act on the pulse rate in fever, as in apyrexia, and reduce its frequency. "In grave cases, however, where the evolution of the disease advanced, and the frequency and weakness of the pulse indicate degeneration of the cardiac fibre, digitalis may lower the temperature, and the pulse remain frequent." (Bernheim, *loc. cit.*)

Did the case of Mrs. A. fall under this category? Certainly not, if the cardiac degeneration which opposed resistance to the characteristic influence of digitalis must be traced to the febrile process. This had not lasted nearly long enough to cause such degeneration. Moreover, it could not be said—at least on the fifth day—that the pulse remained at 100 because it resisted the action of digitalis.

Originally 80 (on the third day) and very weak, it rose to 100 in a few hours after the administration of the digitalis, and at the same time became much stronger. In this state it remained until the moment of collapse, when it became very feeble again, but neither gained nor lost in frequency.

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[1] These doses probably indicate an inferior quality of the herb employed.

As I never saw the patient before the fifth day of the fever, and as the attending physician had never seen her previous to the third day, there is no observation of the state either of the pulse or heart anterior to her acute illness. It could only be surmised that a patient of forty-five, whose pulse, with a temperature of 102°, marked only 80, and this during the first days of the fever, thus long before any febrile degeneration of the cardiac muscle could have occurred, had, habitually, an unusually slow pulse. For the same reasons, it was to be inferred that the indistinctness of the first sound of the heart, noticed at the outset of the fever, had preceded this. A slow, feeble pulse, with indistinct first sound of the heart without valvular murmur, points to nutritive alteration of the heart-walls, tending toward, or even consisting in, fatty degeneration. In this case, the patient would have entered upon the typhoid fever with a lesion identical in effect, though not in precise nature, with one most to be dreaded during the second period of the disease, but then due to the prolonged influence of the infectious agent and of high temperatures; that is, degeneration of the heart muscle.

In this condition there is certainly reason to fear that the molecular nutrition of the heart, already impaired, will be ravaged with exceptional facility by the injurious influences of the fever, which (both infectious poison and abnormal heat) tend to prevent the movement of assimilation in muscular fibre, by accelerating that of disassimilation.

How should digitalis act in such a case?

Theoretically, this important question must be answered by reference to two factors: the action of digitalis in apyretic cases of fatty or dilated heart; the relation of its antipyretic action to its pulse action. Empirically, the results of digitalis treatment of fever must be compared, in cases with and without symptoms of primary cardiac degeneration, and comparison also made between those cases where the medicine is given in the second period of the fever, when the heart is liable to be degenerated, and those where the treatment is adopted during the first period, with an intact cardiac nutrition.

Grimshaw particularly praised digitalis in the continued fever (typhus) where marked cardiac weakness existed in the beginning, or even when fatty degeneration preceded the disease. Thus in his

Case 2.—Typhus in sailor, occurring just after recovery from a prolonged illness. The heart was found very weak on the sixth day. From the sixth to the fourteenth day the patient received infusion of digitalis, at first ℥ ss, then ℥ ij every third hour. The temperature continued to range from 105° to 103°, but the pulse tension was quickly restored, and the heart sounds gained in strength. The number of pulse beats is not recorded.

Case 3.—Eighth day, "heart sounds of fœtal character"; Pulse 136, low tension; 105.2°. Infus. digitalis ℥ i every third hour.

From eighth to eleventh day, tem. varied from 105° to 103° then fell. Pulse fell on ninth day (twenty-four hours after digitalis) to 120. On eleventh day digitalis increased to ℥ iss; convalescence on twenty-first day.

Case 8.—Woman, æt. forty-nine; had had thirteen children. Pulse small and weak, first sound indistinct; much mental anxiety. On sixth day ℥ i infusion every third hour.

On eighth day first sound of heart almost absent. ℥ i whiskey given daily.

On ninth day ℥ i liquor strychniæ added to each dose of digitalis, and latter given every second hour.

On twelfth day heart sounds failed.

Thirteenth day, "pulse tracing that of fatty heart; tends to squaring of apex."

The further details are not given, nor, as seen, is the record of the pulse, complete, but the patient recovered, and the author considered the case a valuable illustration of the power of digitalis as a vascular stimulant. The temperature was never above 102°, a circumstance which he attributes to the fatty heart.

Dr. Grimshaw concludes from his own experience, that digitalis has no influence on the temperature; but is very valuable to prevent or diminish delirium (dependent on imperfect circulation of the brain), to improve the tension and slightly diminish the frequency of the pulse, to increase the power of ventricular contraction, and prevent the loss of vascular tension.[1]

In one of Bernheim's cases, already quoted, the patient began to be cyanosed after interruption to the digitalis: On this symptom of heart-failure, the digitalis was resumed, and the cyanosis disappeared.

Out of one hundred and fifty of Bernheim's cases (cardiac?) collapse was only observed three times. It was always readily dissipated by stimulants. The patients in whom it was observed had taken 0.75 on three successive days, or 36 grains in all. Thus, the dose was rather below that which was borne, in the great majority of cases, with perfect impunity. "Digitalis is quite inoffensive unless the heart is degenerated by the disease, but must be suspended at first sign of collapse." "When integrity of cardiac muscle is suspected, renounce digitalis, or associate it with cinchona or rum" (Bernheim, *loc sit.*, pp. 357, 358). Nevertheless, Wunderlich, like Grimshaw, notices the especially beneficial effect of digitalis "when there is danger of cardiac exhaustion" (*loc cit.*).

"The dicrotism of the pulse is not altered; but a small and miserable pulse becomes persistently fuller and stronger.

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[1] *Dublin Quarterly*, 1873.

The main indication for digitalis in typhoid is excess of temperature, with rapid and weak pulse, and delirium. It is absolutely free from danger " (Hankel, *loc cit.*).

Hirtz declares that the first period of typhoid fever, dependent on the primary typhic process, is alone suitable for the digitalis treatment. That the latter is of no use in the secondary fever of ulceration and absorption, and that digitalis is contra-indicated in cases of moderate fever and adynamic type. But he is only considering the drug as an antipyretic; and lays all the more stress on this action, because he attributes almost all the dangers of typhoid fever to the excess of temperature.

The apparent contradictions in the foregoing statements, which could be multiplied by multiplying authorities, may be reconciled by the consideration that there are at least two forms of cardiac weakness, both manifested by feebleness of the ventricular systole. In the first, the cardiac muscle contracts feebly because its structure is altered, either by the vitreous degeneration, due to the febrile process, or by a fatty degeneration, or even through simple anæmia, which may have preceded this. In the second case, the feebleness is due to deficient innervation, when the excited motor ganglia are overwhelmed by the typhic poison, or exhausted by the long-continued super-stimulus of high temperatures. In either case, the feeble pulse may be either excessively frequent, or abnormally slow. Slowness of pulse may, at any time, be caused when the excited motor ganglia of the heart are overwhelmed by a poison in the circulation, and thus is not infrequent in diphtheria. It seems, however, to be very much less frequent in typhoid, where the feeble pulse is habitually also very rapid,—except in degenerated heart.

In one of Grimshaw's cases, the radial pulse, on the thirteenth day of the fever, yielded a tracing resembling that of fatty heart. This was obtained, however, after the patient had been for seven days under the influence of very large doses of digitalis, and the question suggests itself whether the excessive cardiac feebleness were not due to this.

The mass of testimony offered by physiological observations supports two parts at least of the theory of Traube concerning the action of digitalis on the heart. This drug stimulates the inhibitory apparatus, the vagus, and intra-cardiac ganglia,—as may be shown by the increased susceptibility of the vagi to faradization after its administration, and by its failure to act after this apparatus has been paralyzed by atropine.[1]

But in addition, in apyretic states, the digitalis directly stimulates the contractility of the cardiac muscle. In a frog poisoned by woorara, when voluntary movements have disappeared, and the movements of the heart become more and more feeble, injection of a certain dose of digitaline will be followed by an increase in their vigor.[2] Boehm has measured the work done by the frog's heart isolated from its nerves, when digitalized serum is passed through it, and has found that this is increased,—with a certain dose,—while the number of beats is diminished. With large doses the heart is arrested in diastole. Vulpian found that large doses paralyzed the cardiac muscle, causing irregular and incomplete contractions of both auricles and ventricles.[3]

It is this second action of digitalis which is liable to fail in febrile diseases, on account of the degeneration of the cardiac muscle, which can no longer, therefore, respond to the stimulus. The case resembles the apyretic heart diseases in which the heart fibre has become fatty. So long as the inhibitory ganglia remain intact, the pulse may still be slowed by the digitalis; but neither its force nor the arterial tension will be increased.

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[1] Boehm: Ueber den Herzen Gift. Ackerman: *Sammlung klinischer Vorträge*, No. 48, 1872. We have ourselves repeated these experiments, arriving at the same results.

[2] Gourvat: Thèse Inaug., 1871 (quoted by Bernheim, *loc. cit.*, p. 278).

[3] *Mem. Soc. Biol.*, 1865.

Theoretical Conclusion.

These data accord with the clinical experience that digitalis is predominantly and almost exclusively useful in the first period of typhoid fever,—that is, when the heart muscle and cardiac nerves are intact.

The slackening of the pulse is useful only in so far as it is associated with increased force of the ventricular systole, with prolongation of the diastole, higher vascular tension, and more vigorous capillary circulation. The prolongation of the diastole is the circumstance the most important for the nutrition of the heart itself, since it is during the diastolic pause, or moment of cardiac rest, that molecular assimilation is carried on in the cardiac muscle.

In Mrs. A's case the administration of digitalis was followed by a rise of pulse frequency from 80 to 100, while at the same time the beats increased markedly in strength. This was observed very distinctly on the third, and also on the tenth day of the illness, and is contrary to the usual effect expected. How may it be explained? A slight acceleration of the pulse, preliminary to a fall, has sometimes been observed in physiological experiments with a weak dose of digitalis.[1] But then the pulse becomes at the same time weaker instead of stronger; whereas, in Mrs. A.'s case, the increase of strength was the most important result.

Thomas (*loc. cit.*) observes that occasionally,—just when the pulse is markedly depressed by digitalis, even to the norm,—a sudden increase of frequency occurs. He explains this by reference to the fact that during the digitalis narcosis influences continue to exist tending to accelerate the heart's action, and which, from time to time, may overpower the effect of the digitalis.

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[1] Hirtz: *Bull. de thérap.*, 1862.

When the pulse is rapid and feeble in apyretic heart disease, *e. g.* mitral insufficiency, the rapidity is due to the lowered tension of the arterial system. The nutrition of the heart is enfeebled by the shortness of the diastole, which permits too short a period for nutritive assimilation; and the ill-nourished cardiac muscle contracts, therefore, feebly. Prolongation of the diastole by means of digitalis meets, therefore, all the necessities of the case, so long as the integrity of the peripheric vessels is preserved, and as the lowered arterial tension is itself, by a vicious circle, due to the enfeebled action of the heart.

The constant influences tending to modify the pulse during fever are quite different. Apart from the increased temperature, the infectious agent stimulates the excito-motor ganglia at moderate doses, or when their vital resistance is strong; depresses and tends to paralyze them in the converse case.

"In typhoid fever the pulse is not always proportioned to the temperature, which may be very high, while the pulse remains normal. Very severe cases, with 40 and 41 degrees of temperature (104° and 105.8°) may run their entire course without the pulse reaching 100. In one of my observations the temperature rose to 41.5° (106.7°) when the pulse was 72, and another where it stood at 41° (105.8°) when the pulse was 68." [1]

"There are cases where throughout the illness the pulse is 60 or 80, notwithstanding considerable elevation of temperature." [2]

"Does this relatively low rate of pulse in abdominal typhus depend on the gradual rise of the temperature, or must we admit a depressing action on the pulse of the typhic poison? The latter seems probable from the fact that in apyretic forms, or with moderate fever, the frequency of the pulse is often below normal."[1]

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[1] Bernheim, *loc. cit.*, p. 339.

[2] Griesinger: "Die Infections Krankheiten."

The following combinations of circumstances may occur:

Pulse Accelerated (*moderately*).

I.—Inhibitory and excito-motor ganglia both intact; average dose of fever poison; stimulation of excito-motor ganglia beyond average restraint of inhibitory.

Pulse Accelerated Excessively, and Feeble.

II.—Paralysis inhibitory of ganglia by fever poison and temperature; low arterial tension adding mechanical influence.

Pulse Slowed and Feeble (*only out of Proportion to Temperature*).

III.—Excessive initial dose of fever poison.

IV.—Anterior imperfection in nutrition of excito-motor cardiac ganglia or of cardiac muscle; hence failure to respond to febrile stimulus, while nutrition is directly depressed by fever poison.

Pulse Slowed Below Norm.

V.—Overwhelming dose of fever poison; generalized paresis of cardiac nerves, both excito-motor and inhibitory.

The cases in which digitalis is usually recommended in fevers come under class I. Mrs. A.'s case belonged either to class III or IV. We think the action of digitalis in her case (during the initial period of its administration) can only be explained by the following hypothesis: The inhibitory nerve of the heart responded to the stimulus, and the diastole was prolonged, thus facilitating an improved nutrition of the heart muscle. At the same time this muscle responded to the direct functional stimulus of the digitalis; and under the double influence the force of ventricular contraction improved. A simultaneous improvement in the nutrition of the intracardiac ganglia rendered them more susceptible to the febrile stimulus, and thus the number of nerve discharges or impulses to contraction were increased. The influence of the stimulated inhibitory apparatus, which was inadequate to restrain the number of discharges from the excito-motor ganglia, was manifested in shortening their duration; in other words, the diastole was prolonged at the expense of the systole.

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[1] Liebermeister: *Ziemssen's Handbuch*, Bd. iv., Th. 1.

Sphygmographic observations would have demonstrated whether such modification in the relative duration of the two periods of the heart-beat, really took place. It was inferred, because the force of the heart was indubitably increased, and there is not the slightest reason to suppose that the action of the heart on the cardiac muscle can be exercised without its simultaneous and most characteristic action on the inhibitory apparatus.

The following schema illustrates this hypothesis, which, indeed, is scarcely more than the statement of the facts observed. Since the number of beats before and after digitalis was respectively 80 and 100, they stood to each other in the relation of 8 to 10.

Before digitalis. Pulse 80, feeble. Systole and diastole equal.
| S. D. | S. D. | S. D. | S. D. | S. D. | S. D. | S. D. | S. D. |

After digitalis. Pulse 100. Diastole longer than systole.
| S. D. | S. D. | S. D. | S. D. | S. D. | S. D. | S. D. | S. D. | S. D. |

During the first two days of digitalis treatment, the effect on the heart was unquestionably beneficial. Nor, as has already been pointed out (pp. 38, 39), could the collapse of the fifth day be attributed to the direct action of digitalis on the heart, because in digitalis collapse the pulse always becomes abnormally slow, and the slowing of the pulse precedes the collapse by a period of hours or even days,—whereas, in Mrs. A's case, the pulse remained at 100, though becoming feeble and even irregular.

The feeble pulse in the crisis of the fifth day was, however, only one element of collapse. The brusque defervescence was another,—at least as important; and a superficial reference to the fact, that digitalis has been frequently employed with success as an anti-pyretic, might lead at once to the conclusion that it had caused the fall of temperature in this case.

Assuming for the moment that this were possible, the cardiac depression (of force) could be explained either by the sudden fall of temperature itself (from 103° to 97°, or six degrees), or by the conditions causing that change. The influence of digitalis, therefore, would depend on the nature of its antipyretic action.

The following explanations have been offered:

Theory of Anti-pyretic Action of Digitalis.

That digitalis lowers febrile temperature by acting on the vaso-motor system, causing relaxation of peripheric vessels through whose contraction the elimination of heat has been arrested (Traube).

That it contracts the peripheric vessels, thus causing acceleration of peripheric circulation, and thus more extensive cooling of the surface (Ackerman, Heidenhain).

That the antipyretic action depends on retard of circulation and diminution of arterial tension (Nothnagel).

That digitalis directly diminishes movements of organic combustion (Mégevand, quoted by Bernheim).

That digitalis, like all the medicines which lower the temperature, and keep it lowered during some time, acts directly upon the nervous centres which preside over calorification, and changes the morbid modality in virtue of which the organism regulates its heat at a high degree (Bernheim).

The narrow limits of our purpose, which aims only at the analysis of a single case, does not permit discussion of the obscure and profound questions involved in any of the foregoing hypotheses. It suffices to notice, as has already been done in many criticisms upon them, that an increased elimination of heat cannot suffice to arrest the febrile process, since, in fever, the elimination as well as the production of heat, is already greatly above the normal standard; and that the antipyretic effect of digitalis cannot be explained by its action on the heart, because the first effect is not at all proportioned to the second.

Anti-pyretic Collapse.

On the other hand, any agent that really, and for a long time, depresses febrile temperature, *must* affect the increased organic combustions associated with the fever. The elimination of urea would not, however, be at once diminished, because that which had resulted from anterior retrograde metamorphoses of tissue would for some time be in excess of the norm, even when these metamorphoses had been arrested. Whether the antipyretic action be exerted directly on the molecular nutritive processes in the tissues, or indirectly upon them through the medium of the nerve-centres, it is certain that a profound perturbation must always be introduced into these processes. This, if carried beyond a certain limit, may so far reduce the nutritive exchanges of the tissues, that the vitality of the latter become temporarily impaired. Hence the collapse, of varying degree, which is always liable to attend every artificial, and even every spontaneous, defervescence.

Collapse depending on a generalized depression of molecular nutrition, involves the heart in two ways. The nutrition of the heart is perturbed, together with that of all other tissues, and the capillary circulation is retarded more and more, thus offering increasing obstacle to the work of the heart, until this is liable to be arrested by the peripheric resistance.

To these dangers must, in the case of digitalis, probably be added the danger of direct paralysis of the cardiac muscle, liable to be induced as a secondary effect, to its original functional stimulation by this drug, and especially with the excessive doses habitually required to depress the temperature.

In Mrs. A's case these doses were not used. The only possible way in which the digitalis could have been responsible for the collapse of the fifth day, would have been by accumulating in the organism. But forty-eight hours was certainly too short a time for such an accumulation to be effected. We must conclude, therefore, that the defervescence, with attendant collapse, must have been spontaneous. Such early collapse is always ominous, and often indicates an unusual extent of intestinal lesion. Hamerynk describes cases where *all* the glands of the intestine, from the duodenum to the rectum, were infiltrated, and death occurred with choleriform symptoms.—(Quoted by Griesinger.) In Mrs. A's case, diarrhœa did not set in till the ninth day, thus four days after the first attack of collapse. But a generalized infiltration of the small intestine may have begun much earlier. The second attack of collapse followed the first onset of diarrhœa; and this fact makes it the more probable that the first originated in extensive irritation of the abdominal nerves from the typhous infiltration. In this case the irritation must have been reflected at once to the nerve-centres, overwhelming them and depressing the temperature; and to the intra-cardiac nerves, weakening the force of the ventricular contractions,—hence of the pulmonary circulation, rendering imminent pulmonary hypostasis.

The long apyretic period which followed the first collapse, although the heart's action regained its force and the pulmonary congestion diminished, showed sufficiently that the cardiac and nervous collapse were independent of each other.

And now the important practical question presented itself,—and even more urgently in reviewing the case than in watching it,—whether, during a defervescence that had occurred independently of the action of the digitalis, the usual rule, which requires suspension of the drug as soon as the temperature falls, should be observed. It was assumed that the small doses would act as a cardiac tonic; and as the number of beats had not been depressed, there seemed no reason for discontinuing them. A further rule, laid down by Wunderlich, Hirtz, and Bernheim, is that if a very rapid pulse continues to resist the digitalis, the drug must be abandoned, as it is then liable to become dangerous. But Mrs. A's case did not seem to come under this rule, for the number of beats was not excessive, being only 100.

The point of possible danger lay in the possibility of slow accumulation of the drug in the system. To us it still remains uncertain, how far this danger was really incurred in Mrs. A's case. It is, therefore, also a question, and a grave one, how far the remarkable succession of attacks of collapse, which marked the second period of the fever, indicated a struggle of the heart with a remedial poison, in addition to the poison of the disease, or whether the heart was sinking in spite of the best aid which could be afforded it. It was, of course, the latter supposition which led to the continuance of the digitalis. On the affirmative of this question must be reckoned: 1st. The known tendency of the drug to accumulate; 2d. The cyanosis which accompanied the sinking attacks, becoming more pronounced as these were more frequent. Cyanosis is a marked feature of digitalis collapse. It seems to depend on the accumulation of blood in the veins which results from the low arterial tension.

On the other hand, an unfavorable influence of digitalis seemed to be negatived by: 1st. The immediate increase in cardiac force that followed its administration on the night of the 10th, after an interruption of several hours; 2d. The increase in the number and severity of the collapse attacks during the four days (from the 13th to the 17th) that the digitalis was suspended. A favorable influence seemed also to be inferred from the diminution of the pulmonary symptoms under its use. The pulmonary hypostasis at first threatened was averted, and although a limited pneumonia subsequently developed, it did not seem to determine the fatal issue. At all events this pneumonia was under the direct influence of the typhic poison, not of the heart failure.

In regard to the latter point, however, it is probable that the interruption was not sufficiently prolonged to really test its effect, since the effect of digitalis persists for more than four days after cessation of a dose that can be shown to have produced one. As I myself only saw the case at intervals, and only five or six times altogether, I cannot tell what impressions might have been derived from a more continuous observation. It is noticeable from the chart that on one occasion at least, the pulse fell to 66, and this certainly should have been a signal for interrupting the digitalis. But when, on the contrary, the medicine was continued, the pulse rose again.

A case treated with digitalis by Sydney Ringer, and published by Morell in the *Practitioner* for 1873, has an important bearing on the dangers of the possible accumulation of even moderate doses. The patient was a young girl, and no cardiac degeneration existed previous to the fever. The drug was administered continuously, at first in doses of one grain of the powder; afterward in doses of nine, then twelve, minims of the tincture. The dose was increased when the pulse became more feeble, and the medicine continued after the frequency had disappeared; an evident error. The patient died suddenly in syncope.

The above case is of very great importance for our purpose, since, except in the age of the patient, it bears a considerable resemblance to Mrs. A.'s.

Liebermeister, in *Ziemssen's Handbuch der Medicin*, and later in the *Handbuch der allgemeinen Therapie*, declares his dissatisfaction with digitalis as a remedy for the reduction of a rapid, weak pulse, with high temperature, in typhoid fever. He thinks it generally fails, and sometimes even *seems* to accelerate the cardiac paralysis.

Bartholow, in the last edition of his Treatise on Materia Medica and Therapeutics (1884), quotes rather summarily from Liebermeister, and, on the authority of the latter, throws over the use of digitalis completely in the emergency described. Murchison says he has no experience whatever with it.

The foregoing case has been analyzed in detail, because it is thought that in this weighty matter, when such important authority may be quoted on both sides, that a single detailed clinical observation is of value. If the observation does not positively demonstrate a dangerous influence exercised by digitalis,—it must at least be counted as a case in which this "cardiac tonic" altogether failed in the very emergency for which it has been so warmly recommended.

Taken together with the case of Sydney Ringer's, ours probably illustrates the truth of the dictum of Hirtz, that small doses of digitalis given for a long time are more dangerous than large doses given at the outset of the disease for a short time. It may be inferred, also, that if small doses are given, without apyretic intention, but as a cardiac tonic, it would be better to give them according to the method in use in chronic heart disease: namely, ten to twenty drops of the tincture three times a day. Further, that in any case,—*i. e.*, whether the medicine seemed to be doing good or not,—it should be suspended every three or four days for a period of four or five days. A shorter period does not suffice to test the effect of the interruption, since the influence of digitalis on the pulse, though not on the temperature, remains perceptible during four of five days after the cessation of its administration.

Out of the fifty-three cases of typhoid fever treated with digitalis by Bernheim, eight proved fatal. In three cases death was due to an intestinal perforation; in three the digitalis was given *in extremis* in an advanced stage of the disease, when the heart was fatty and pulmonary hypostasis already established; in one case the patient was alcoholic, with fatty heart; the eighth case succumbed to intestinal hemorrhage.

" When the cardiac muscle is softened and the lungs engorged, it is too late for digitalis; this drug is even formally contra-indicated, because, if it acts at all, it will be to slacken the heart without reinforcing it; it may, therefore, precipitate a fatal collapse" (Bernheim, *loc. cit.*, p. 437).

" Nor," continues this writer, who, with his master, Hirtz, is one of the most enthusiastic advocates of digitalis, " would I affirm that whenever digitalis is given in time it will prevent the heart from degenerating. In typhoid fever we are never able to count *with certainty* upon the efficacy of digitalis: the antipyretic action, like that on the pulse, may entirely fail, or be only temporary. But the *most often* this influence is sufficiently durable to avert one of the greatest dangers of the disease, that which results from cardiac feebleness" (p. 438).

To this statement we must add: When that feebleness depends on the influence exercised on the cardiac nerves by the febrile process, and is unassociated with structural alterations, either caused by the disease or anterior to it; when, also, the cardiac weakness is associated with high temperature; when, as in Mrs. A's case, the temperature falls, digitalis, even in small doses, seems likely to be useless, if not injurious. It is possible that it prove injurious, as in the fatal cases of Ferber and Ringer (see above), when fall of the pulse was no more marked than in favorable cases, or when it did not occur at all, as in ours,—when, also, vomiting, a most characteristic sign of digitalis narcosis, is absent.

For this drug, as for chloral, morphine, and many others, prolonged resistance on the part of the organism to its characteristic effects should be a warning not to push, but to cease, the administration, lest cardiac collapse occur suddenly.

Apart from the influence of digitalis, the observation illustrates two facts in typhoid fever: 1st. The extreme difficulty of combating the symptom of heart failure, implying failure in the nutrition of the heart muscle, by either general or special stimulants; 2d. That the danger of typhoid fever is by no means exclusively proportioned to the rise of temperature. Since, in the absence of high temperature, the cardiac depression and generalized nutritive collapse must be due to the febrile poison alone, and since this agent is constantly present, together with high temperature, when that exists, it is impossible to ascribe either the direct or indirect symptoms of heart failure exclusively to the temperature, even when this is high. We may repeat the question, which has already been asked, how far the value of artificial depression of temperature by antipyretic treatment, and especially by cold baths, does not consist in a temporary interruption to the multiplication of the infectious agent in the organism.

A CONTRIBUTION TO HERNIOLOGY.

By ARPAD G. GERSTER, M.D.,

SURGEON TO THE POLYCLINIC, AND TO THE GERMAN AND MOUNT SINAI HOSPITALS.

THE cases here presented possess sufficient importance to be brought to public notice. One illustrates a rather unusual cause of failure of a timely herniotomy ; the second, the occasional necessity for resorting to the most ancient form of radical operation of hernia, viz., that supplemented by castration ; the last one a normal operation of a rather complicated case followed by the radical operation, as practiced by German surgeons at the present day.

Case 1.—Joseph Mandelbaum, a well-preserved man of seventy-three years, had had an oblique inguinal hernia on the right side for twenty years. The patient was always able to replace the tumor except on two occasions, when the aid of a physician was required to accomplish reduction. He had worn a truss since ten years. The tumor had come down thirty-eight hours before his admission to Mt. Sinai Hospital, on Aug. 21, 1883, and a physician had attempted taxis unsuccessfully. Very severe abdominal pain, nausea, with occasional vomiting, great excitement marked by volubility of speech, and the absence of alvine evacuations gave the case a rather grave aspect ; but the temperature being normal, and in the absence of tympanites, or signs of local inflammation about the tumor, it was decided to administer a generous dose of an anodyne, together with an ice-bag over the swelling.

During the subsequent seven hours the patient's thirst becoming unquenchable, and his restlessness excessive, it was decided to try operative relief. The tumor was four inches long, and about three inches wide, not painful on touch, its tension moderate, and it gave resonance on percussion. Ether being administered, a gentle attempt at reduction was made, without success, whereupon the sac was exposed by a suitable incision extending well up to the external abdominal ring. By invaginating the tip of the finger into the inguinal canal, a few fibres connected with the internal pillar were felt to present an obstacle to reduction. These being divided, the contents of the sac could be readily slipped back into the abdominal cavity without opening the hernial sac. The wound was irrigated with a 1:2000 solution of bichloride of mercury, was closed by a few silver-wire sutures, and dressed with a strip of iodoformed muslin.

Six hours after the operation a temperature of 99.5° F., with 98 pulses, was recorded. Within six additional hours fever came on, with delirium and deep depression, the pulse not rising in proportion to the temperature. The former did never exceed 105 beats, and was mostly varying between 75 and 100 per minute ; the temperature, however, gradually rose to 104.4° F. and remained there until the patient's death, which took place thirty-nine hours after the operation. The wound showed no reaction throughout, and although the patient complained of pain in the belly, pressure did not increase it, and there was no meteorism whatever.

The post-mortem examination made four hours afterward revealed the following facts : Primary union of the external wound, which being forcibly opened, about a drachm of bloody serum was seen to collect in its bottom. The sac being incised, it was found to be empty, its serous lining pale and smooth, and through the inguinal canal the finger was passed into the abdomen, and the abdominal wall was divided. The peritoneum was found normal ; in the bottom of the pelvis were a few drachms of limpid serum. The intestines presented a normal, pale aspect, except about three feet of ileum, which appeared rosy, and slightly distended by gas. This being gradually raised out of the belly, a well-circumscribed, slate-colored and shrunken part of the gut came to view, extending over two and a half inches of the convexity. Corresponding to this necrosed portion of the ileum, an old and firm cicatricial cohesion of several coils of intestine was found to exist, undoubtedly due to a former inflammatory process. Here the mesentery was thickened and œdematous, showing purplish discoloration, and *a number of hard and much enlarged mesenteric glands* were found to form the nucleus of this dense cicatricial tissue. There was no adhesion to the abdominal parietes. *Cause of death:* Septicæmia due to necrosis of ileum.

It seems probable that necrosis was produced more by compression of a terminal mesenteric artery of the gangrenous knuckle, through undue and prolonged traction, than by direct strangulation of the gut. This view is justified by the slight impediment requiring cutting, and that the cause of incarceration was found outside of the sac.

Furthermore, it is fair to assume, that necrosis of the gut had commenced before the operation, producing symptoms of pain and depression, but that decomposition and absorption of septic matter must have set in later than six hours after the operation, when their presence and that of incipient peritonitis were indicated by septic fever.

It is said that the extra-peritoneal operation, as illustrated by this case, is safer than that accompanied by opening the sac. In the light of the experience furnished by the preceding case, and possessing the aid of antiseptic precautions, we must concede that this view needs material correction. Had the sac been opened in this case, inspection may have revealed the true state of affairs; and excision of, or even a simple incision into, the decayed gut might have saved the patient's life.

Case 2.—August Bentlage, æt. twenty-four, a rather slender metal-worker, whose pale skin was dotted with a number of pigmented moles, was admitted to the German Hospital, August 17, 1883, giving the following history:

He had a hernia since childhood, never wore a truss, and felt no inconvenience whatever until this summer, when he commenced to be troubled by frequent and very sharp griping pains in the belly, which subsided only when he assumed the recumbent posture. Being unable to do work, he begged to be relieved by an operation. On examination, a right oblique inguinal hernia was made out, consisting principally of omentum. It could be readily reduced to a certain point, the testicle following the hernia up to the external abdominal ring. When the testis was pulled down, the finger meanwhile retaining the hernia, unbearable pain and nausea were complained of. On further examination, a band-like mass could be felt running from the anterior aspect of the testicle into the hernial mass. The patient never had tried to wear a truss, wherefore several attempts were made to fit him with one; but all resulted in causing intense pain. The patient was visibly emaciating, urgently demanded relief, and, on Aug. 23d, a hitherto unnoticed knotty swelling was detected, occupying a certain part of the hernia. Malignant disease of the omentum being suspected, the following operation was performed at once: The testicle, together with the hernia and tuberous mass, were well brought down, and the sac was opened. It became plain that we had to deal with a congenital rupture, the omentum and the gut occupying the tunica vaginalis. The omentum was firmly attached to the anterior surface of the testicle, and presented, in its middle, the appearance of a nodular pigmented mass, surrounded by unchanged omental tissue. The inner surface of the sac itself and the testis, were also sprinkled with a large number of lenticular pigmented spots. The testis and cord were somewhat atrophied. First the cord was drawn out, severed, and the vessels tied with a number of catgut ligatures; then the omentum was ligatured and cut off; the neck of the sac was separated from the surrounding tissues, and was forcibly drawn out of the wide inguinal canal, till a place was reached about half an inch beyond the last pigmented spots. Here a purse-string-like suture of catgut was passed in and out alternately, through and around the circumference of the sac, and was firmly tied, the stumps of the cord and omentum having previously been returned into the abdominal cavity. After this the sac was cut off below the circular suture, and was dissected out of the scrotum, in one mass, with the testicle. The external wound was closed with a few stitches; no drainage-tube was inserted.

No local or general disturbance followed the somewhat severe operation, the wound healed by adhesion, and three weeks after the operation the patient could be permitted to leave his bed, using a truss with comfort. Notable were afterward a pertinacious constipation and gastro-intestinal catarrh, requiring internal treatment till Nov. 26th, when he was discharged cured, and in excellent health.

Cut into, the nodular mass was found to be composed of a mottled, dense, sarcomatous tissue. The vas deferens and vessels of the cord were rather slender, but the cremaster showed a remarkable state of hypertrophy, due, without doubt, to the constantly provoked reflex phenomenon of the testicle being tightly drawn up to the inguinal canal.

In this case rapid growth and local dissemination of a pigmented sarcoma of the omentum was caused by the pressure of a truss. The sarcoma had originated in cicatricial tissue, produced by a former local peritonitis.

CASE 3.—Josephine Reinhardt, fifty-four years old, married ; a well-nourished, hard-working woman ; has had, since many years, a large omental hernia of the left side, never wore a truss, and exhibited symptoms of incarceration since three days. On November 15, 1882, stercoraceous vomiting set in, and Dr. Guden requested me to see the patient. I found her pulseless, the skin covered with a clammy perspiration, the fetid vomiting almost constant. The integument over the hernia was much suggillated : the effect of repeated unsuccessful attempts at taxis. The case admitted no doubt, and the patient was at once brought under the influence of chloroform. During the struggle of the stage of excitation the hernia suddenly enlarged in size by about one third, the skin over it becoming tense and shining. A cutaneous incision of seven inches laid bare the sac, which being opened about half an ounce of bloody serum escaped. A mass of hypertrophic omental tissue, the size of a man's fist, became thus visible, which being deligated in seven portions with stout catgut, and cut away, a small knuckle of purplish-blue lustreless gut was exposed. The strangulation was found to be situated within the external inguinal ring, which on being incised, offered no resistance to the replacement of the omental stump. The gut was drawn out well for inspection, and its reflex contractility on pinching still being preserved, I deemed it advisable to replace it in spite of its bad appearance. The gut and the sac were well washed with strong carbolic solutions, then the former was slipped into the abdominal cavity. The sac was dissected out, up to its neck, and was cut off below a purse-string suture encompassing the neck of the sac and the external abdominal ring. After this the large external wound was united with a number of catgut sutures, a small drainage-tube being placed in the lower angle of the wound. An antiseptic dressing and spica covered the whole. A hypodermic morphine injection was given, and the pulseless patient was put to bed. The vomiting had entirely ceased, the pulse rallied, and a large quantity of flatus passed during the night. The temperature rose to 101.5° F. the next morning, but her general condition being subsequently satisfactory, the dressing remained undisturbed. Some trouble was experienced in establishing a free movement of the bowels, but this having been accomplished on the fifth day, she could be declared as convalescent. Catheterism, which was needed until then, also became unnecessary. The first dressing was changed November 20th, and with the exception of the opening left by the drainage-tube, the entire wound was found healed by first intention. The next change of dressings occurred November 28th, when a firm cicatrix was found in the whole extent of the incision. Five months later I found, on removing the truss, no hernia, and very little of impulse on coughing, but advised to wear the truss constantly.

This case may be accepted as a fair illustration of the modern radical operation as practised in Germany. It does not accomplish a radical cure in such a sense as to obviate the necessity of wearing a truss. Experience has shown that in these so-called cured cases a relapse is apt to occur on slight provocation, after the usual shrinking and softening process of the cicatrix has been well advanced. But its advantages are nevertheless so great that, whenever practicable, this slight addition to the usual herniotomy should not be omitted. After it the patient gets along very well with a light truss, which also is amply sufficient to prevent a recurrence of the rupture.

In most cases the dissection of the sac is very easy; I generally succeeded in stripping it out of the surrounding connective tissue without the use of a cutting instrument. It is proper to remark, however, that some cases are on record in which the dissection of the sac was reported to have been very troublesome.

MORPHIA AND GELSEMIUM.

By ISAAC OTT, M.D.

As is well known, some drugs when given together increase the action of each other, whilst others antagonize. Thus the preliminary use of morphia prolongs the effect of chloroform, whilst atropia antagonizes the activity of the first.

In this paper I shall study the effect of a previous dose of morphia on the toxic activity of gelsemium, whether it accelerates it or retards it.

Gelsemium for its poisonous virtues depends on two bodies, gelsemia and gelseminic acid, the latter being mainly a convulsivant. That gelsemium is a very powerful agent is attested by the number of cases of poisoning resulting from it, many of them fatal.

In one case of poisoning the subsequent use of morphia was thought to have acted beneficially. In this paper I shall try to show that the preliminary use of morphia increases the rapidity of the toxic action of gelsemium. The experiments were made with a fluid extract of gelsemium, and upon kittens either of the same litter or as nearly as possible of the same size. The weight of the animals is in pounds.

Expt. 1.—Kitten received at 2.1 p.m. one half a grain of morphia subcutaneously. At 2.10 p.m., twelve minims of the fluid extract of gelsemium ; the animal is very lively, rotates in a circle ; posterior extremities weak. 2.28 p.m., twelve minims of fluid extract of gelsemium. 2.30 P.M., convulsive movements. 2.43 P.M., difficult respiration. 2.55 P.M., kitten dead; when the death agony commenced it was rapid. Weight of animal, $1\frac{15}{50}$ pounds; death in forty-five minutes.

EXPT. 2.—Kitten of same litter as the one used in expt. 1; received thirty-four minims of fluid extract of gelsemium. 2.15 P.M., lying down, trembling about the lips and head. 2.28 P.M., twelve minims of fluid extract of gelsemium. 2.31 P.M., trembling movements of head. 2.55 P.M., difficult respiration; head falls between the anterior extremities. 3.12 P.M., death slow in its nature. Weight, $1\frac{9}{50}$.

EXPT. 3.—Kitten received subcutaneously one half a grain of morphia at 1.15 P.M. 3.25 P.M., limbs weak; when he attempts to walk the limbs spread apart; animal is dazed. 7.35 P.M., in dazed condition; does not know what he is about; recovered next day. Weight, $1\frac{9}{50}$ pounds.

EXPT. 4.—Kitten at 3.15 P.M. received one half a grain of sulphate of morphia. At 3.25 P.M., twenty-four minims of fluid extract of gelsemium. 3.32 P.M., circus movements. 3.35 P.M., lies on his sides, utters cries; inco-ordination; pupils dilated; claws with extended paws in order to move. 3.40 P.M., can raise his head up, rotates on his long axis. 4.5 P.M., cat is able to cry and struggle; muscular tonus is good, although respiration is irregular; jerking inspiration. 4.15 P.M., dying, although able to move his limbs. 4.16 P.M., death. Weight, $1\frac{48}{50}$.

EXPT. 5.—Kitten at 2.30 P.M. received twenty-four minims of fluid extract of gelsemium. 3.35 P.M., seems unconcerned, and sits upright as usual. 3.43 P.M., trembling about the muscles of the neck. 4.15 P.M., lies with head on the floor, as if asleep; can walk very well; muscular tonus lost; muscles soft. 4.16 P.M., can run over the floor; great difficulty in keeping the head elevated, much more so than in cases where morphia had previously been used; muscles greatly relaxed; cannot struggle violently, or raise himself upon his feet. Death at 4.43 P.M. Weight, $1\frac{43}{50}$.

EXPTS. 6, 7, 8.—Three kittens received at 1.30 P.M. half a grain of morphia. At 1.45 P.M. they received twelve minims of the fluid extract of gelsemium subcutaneously. At 2.10 P.M. all were profoundly affected; inspiration quick, difficult; expiration easy.

Death of 1st at 2.32 P.M. Weight, $1\frac{47}{50}$ pounds.
" " 2d " 2.34 " " $1\frac{44}{50}$ "
" " 3d " 2.43 " " 2 "

EXPTS. 9, 10.—Two kittens received at 2.45 P.M. twelve minims of fluid extract of gelsemium subcutaneously. 3 P.M., walking about; drink some milk. First died at 4.9 P.M. Weight, $1\frac{29}{50}$. The second cat recovered, although profoundly affected by gelsemium. On the following day the same cat, at 1.15 P.M., received a half grain of morphia, and immediately after it, twelve minims of the fluid extract of gelsemium. Death at 2.10 P.M. Weight, $1\frac{46}{50}$ pounds.

In examining the above experiments, the results will be found as follows:

Expt. 4,	morphia, ½ gr.;	gelsem.,	24 minims;	weight, $1\frac{48}{50}$;	death in 51 minutes.
“ 5		“	24 “	“ $1\frac{43}{50}$	“ “ 73 “
“ 1,	morphia, ½ gr.;	“	24 “	“ $1\frac{15}{50}$	“ “ 45 “
“ 2		“	46 “	“ $1\frac{9}{50}$.	“ “ 63 “
“ 3,	morphia, ½ gr.;				did not die.
“ 6	“ ½ “	gelsem.,	12 minims;	weight, $1\frac{47}{50}$;	death in 62 minutes.
“ 7	“ ½ “	“	12 “	“ $1\frac{44}{50}$	“ “ 64 “
“ 8	“ ½ “	“	12 “	“ 2	“ “ 73 “
“ 9		“	12 “	“ $1\frac{29}{50}$	“ “ 75 “
“ 10,	morphia, ½ gr.;	“	12 “	“ $1\frac{46}{50}$	“ “ 45 “

By an analysis of the above experiments, it will be seen that morphia and gelsemium combined kill quicker than gelsemium alone.

A priori it would be expected that two drugs like morphia and gelsemium, both paralysants of the respiratory centres, would accelerate each other's toxic action.

TWO CASES OF EMPYEMA SUCCESSFULLY TREATED BY "THRO' DRAINAGE."

By JOHN J. CONWAY, M.D., and WILLIAM M. THALLON, M.D.,

BROOKLYN, N. Y.

Case 1.—John P. H. Age, twenty-one. Born in Sweden. Shoemaker. Was sent into the Brooklyn City Hospital during our joint service on February 11, 1881, by Dr. Robert Ormiston, the Visiting Physician. Patient on admission was much emaciated, and so weak that he could not walk unaided. He had raised blood several times; had considerable cough and profuse expectoration and frequent night-sweats. On physical examination (by Drs. Ormiston and Conway), the apices of both lungs gave all the evidences of tubercular deposit.

Diagnosis.—Pulmonary tuberculosis.

A generous nutritive stimulating diet was directed. The medication consisted in ol. morrhuæ and tinct. ferri chlor. three times a day, and extr. ergot. fl. according to indications. For the night-sweats he was given tinct. belladonnæ, ♏ vij, with good results.

Patient improved rapidly, gaining flesh, cough and expectoration ceasing, and on the advent of fine weather he was discharged on April 3, 1881.

He was readmitted to the hospital two weeks later (on April 17th) in almost as bad a condition as when he first came there. For the first few days no particular attention was paid to him beyond putting him on his former treatment, it being supposed that he was simply suffering from exacerbation of his old trouble. But having had several rigors, without a corresponding elevation of temperature to indicate an acute febrile condition, he was examined by Dr. Conway, who found the percussion note on the left side of his chest, as high as the third intercostal space, absolutely flat, and above this marked dulness. Pulmonary resonance on this side of the chest could not be detected. The intercostal spaces were bulging and prominent; respiration rapid, and confined to the right side; skin cyanosed, and the patient's expression anxious and distressed. A diagnosis of empyema was made, and confirmed by the introduction of a hypodermic needle and the drawing off of a syringeful of pus.

General treatment having been tried without any benefit, it was evident that in an operation was the only source of hope. He was rapidly failing from day to day; pulse very feeble and rapid, 130 per minute. Respirations increased as high as thirty-six, and he was fast losing flesh. Temperature remained low, often subnormal, dropping a degree and a half (97°), the highest elevation in the week preceding the operation being 100½°.

With the kind consent of Dr. Ormiston, under whose charge the patient was, and after some consultation by letter with Prof. Rushmore as to the method of operating, the following operation was done on May 1, 1881: The lowest point from which pus could be drawn with a hypodermic syringe was found to be between the tenth and eleventh ribs, on a line drawn vertically from the posterior fold of the axilla. Through this intercostal space a large trocar and canula were thrust; the trocar was withdrawn, and as the pus was gradually allowed to flow out, a long probe (a uterine sound served the purpose) was carried through the canula and passed, hugging the chest wall, upward and forward to the fourth and fifth ribs, about two inches to the left of the sternum. The apex beat of the heart was displaced about an inch and a half to the right of this point. The point of the probe being felt through the intercostal wall, was cut down on, and it was then thrust through this incision. The end of the probe having been bent on itself, a good-sized rubber drainage-tube, perforated, was fastened to it, and the whole withdrawn through the posterior opening. The amount of pus immediately evacuated was about fifty-five ounces, and there must have been about twenty ounces more on the dressings next morning. The ends of the drainage-tube were tied together, and the whole retained in place by adhesive straps. Thick wads of oakum, soaked in carbolized oil, were placed on the wounds, and a body bandage applied. The operation was done without an anæsthetic; ether was tried at first, but produced such alarming symptoms of failure of respiration and heart action, that it had to be discarded.

The next day the pleural cavity was washed out with five-per-cent. solution of carbolic acid, but this was soon diminished to

1 in 250. The drainage-tube was twice changed, smaller sizes being used, before it was finally removed. Suppuration diminished day by day, and when the carbolized water came through quite clear, the tube was removed on June 20, 1881.

At first, washing out was done twice a day, but later only once. Patient's temperature never reached 100° except on the eighth day, when it touched 100½° ; respiration at once fell below 30 and gradually became normal ; pulse similarly fell to below 100 and gradually diminished to normal. His improvement after the operation was immediate ; he rapidly regained strength and appetite, and his weight increased upward of twenty pounds in two months. His chest condition when discharged on July 31, 1881, was as follows : Right lung normal ; heart position normal, apparently ; apex beat about a half inch to the inside of the anterior cicatrix and slightly lower. Left lung good ; respiratory murmur: throughout, resonant on percussion, with some dulness below the level of the fifth rib, probably due to pleuritic thickening. The evidences of apiceal deposit had disappeared.

When last seen, in March, 1883, the patient stated to Dr. Thallon that he had worked at his trade steadily since his discharge from the hospital, and that he had not had any further trouble with his lungs or chest.

Case 2.—Johnnie M. Age ten. Born in U. S. Was accidentally shot on May 23d, 1883, with a pistol in the hands of a playmate. The pistol was loaded with a slug of lead. He was removed by ambulance to the Homœopathic Hospital, where he remained for eleven days. Owing to some dissatisfaction he was then taken home by his father, and Dr. Conway was called in on June 7th, fourteen days after the injury. He was hot and feverish and had a temperature of 103½° ; pulse very rapid, and respirations over thirty a minute. Considerable cough and pain in the chest. The point of entrance of the slug could be distinctly seen—it was an inch and a half to the right of the sternum, between the second and third ribs. No point of exit could be discovered, and as no attempt had been made to extract the ball, it is reasonable to suppose it was lodged in the right lung. What made this more than probable was, that at the point of entrance through the cicatrix, one could feel distinct crepitation, and for some distance around the tissues were emphysematous. The whole right pleural cavity below the point of injury was filled with fluid, and the diagnosis was therefore made of traumatic pyo-pneumo-thorax.

The patient's hygienic surroundings were of the worst; he lived in a damp basement, his food none of the best, and his parents ignorant people. The boy's general appearance was markedly tubercular, he had the square-shaped head, the clubbed nails, and the general appearance of that type. He began to rapidly lose flesh and the constitutional symptoms of blood-poisoning, rigors, high fever, sweats, foul tongue, etc., etc., became daily more pronounced. Having tried every thing he could think of in the way of general treatment—without any avail,—Dr. Conway suggested an operation as the only hope of saving the boy's life. To this the parents would not consent, and the patient continued to grow worse. Finally they realized how foolish they had been and urged the doctor to operate, which he reluctantly consented to do (regarding the case as hopeless) on June 21, 1883. We did exactly the same operation as in case 1. There were present: Dr. T. Mortimer Lloyd, who kindly gave chloroform, and Drs. B. F. Westbrook, A. R. Paine, Eddy, etc., etc. The lowest point posteriorly at which pus could be obtained was between the ninth and tenth ribs, about an inch external to the angle of the ninth rib. Instead of plunging in a trocar and canula, a scalpel was used but it did not prove quite so convenient. The long probe was introduced and carried up to and through the point where the ball had entered the chest-wall. A drainage-tube was then introduced and retained by the same manœuvres as in case 1. The amount of pus evacuated at the operation was seventy-two ounces; the pleural cavity was immediately flushed out with a weak solution of carbolic acid; large pads of marine lint were placed over the wounds and kept in place by a snug-fitting roller bandage. Hypodermics of brandy were freely administered and the patient put back to bed. It was the general impression of the gentlemen present that the patient would not have lived another twenty-four hours. He reacted well from the operation, and improvement at once began. The temperature record was not so uneventful as in the first case, for on the fifth day the thermometer marked 103°; occasionally during convalescence there were such exacerbations of temperature, and they did not seem due to any retained pus in the pleural cavity but to some localized suppuration in the connective cellular tissue of the chest-wall along the inferior track of the drainage-tube. At first the pleural cavity was washed out every day, then every second day, and finally every third day. The drainage-tube was gradually withdrawn and finally removed on July 7, 1883, sixteen days after the operation. Pulmonary resonance was then good over the whole right side; the cough and constitutional symptoms gradually disappeared. Crepitation at the point of entrance of the ball could no longer be felt or heard, the slug having probably become encysted. As there was considerable retraction of the right chest-wall and compensatory curving of the vertebral column, the boy was instructed to take forced and deep inspirations, and taught appropriate gymnastic exercises to correct the deformity; this he quickly and satisfactorily did. He was kept on quinine, iron, and digtalis, and the best food obtainable. He rapidly gained in flesh, and as soon as the drainage-tube was removed and the wounds firmly healed, he was sent to the Children's Seaside Home at Coney Island. His restoration to health was there completed, and he is now, six months after the operation, well and strong.

Remarks.—These two operations illustrate very well the success that may be attained in treating these cases under apparently the most unfavorable circumstances if the principal indication—of securing *complete drainage*—is met. It is because aspiration, or the employment of a single opening with a long compressible rubber tube, fails to meet this cardinal indication that they so often prove almost useless.

Another point of great interest suggested by the first case is that the moment the strength of the carbolic-acid solution used in washing out the pleural cavity was markedly decreased, the suppuration correspondingly diminished. This would seem to indicate that strong solutions injected into the pleural cavity tend to keep up the secretion of pus, and that if merely left to itself, or washed out by a solution whose cleansing action is mainly mechanical, the pleura is quite competent to take care of itself.

In other words, the irritation having stopped, the morbid secretion will soon cease.

In this connection it certainly seems difficult to justify the practice of some surgeons who inject caustic solutions of carbolic acid, tincture of iodine, etc., etc., to destroy the pyogenic membrane (?).

We have never heard of any surgeon who proposed to wash out the peritoneal cavity with caustic solutions after a traumatism or the removal of an ovarian tumor, even though purulent and septic accumulations have subsequently occurred. And yet, why would it not be as rational—or rather as irrational—as the similar proposal so often made in the case of empyema ?

Both of these cases illustrate very well the extraordinary reparative power of nature and its tendency to resist pathological processes, and to cope successfully with a most formidable constitutional disease, provided the continuously irritating local cause is removed. That is all the surgeon should try to do, but it should be done thoroughly ; and if it is done thoroughly, nature will do the rest.

EDITORIAL DEPARTMENT.

THE PROGRESS OF THE NON-RESTRAINT SYSTEM.

THE "non-restraint" system of treatment of insanity has now for four years been successfully carried out by me at the Kings County Insane Asylum.

On July 1, 1880, my first paper on the subject was read before the meeting of the National Conference of Charities, held at Cleveland, Ohio, "non-restraint" having then been carried out by me for six months preceding that conference. In that paper, subsequently published in the "Transactions of the National Conference of Charities," I pointed out the practicability of its adoption, the difficulties to be met with and overcome, and the advantages to the patients from such a method of treatment; how patients formerly troublesome, pugnacious, and destructive became comparatively quiet, docile, and industrious. I also pointed out the advantages of *occupation* for these insane patients, and *how it was almost a necessary part of the non-restraint system.* In an editorial in these ARCHIVES in February, 1881, at the completion of our first year of "non-restraint," I again pointed out the success we had met with and our entire satisfaction with the method. During this first year much opposition and adverse and unfair criticism, were offered by a large number of superintendents. Some indulged in sarcasm, and others even went so far as to say that we did not carry out non-restraint.

In April, 1882, another editorial in these ARCHIVES appeared giving our second year's experience with non-restraint, and also meeting a statement which had been quite generally made and quoted by superintendents, to the effect that if we had abandoned restraint apparatus we used a large quantity of drugs, and "chemical restraint" was substituted for mechanical restraint in all "non-restraint" asylums. In that paper there were reproduced some tables drawn up by the late Dr. Wilbur (ARCHIVES, vol. vi., p. 271) from statistics of asylums in Great Britain, Canada, and the United States, and these tables fully showed that the allegation was unjust and unfounded; but, on the contrary, with non-restraint less sedative drugs were used.

It is now the closing of the fourth year of the non-restraint system in the Kings County Insane Asylum—a length of time which fully justifies the assertion that it is now clearly established in this country that non-restraint is practicable, and is a beneficial method of treatment. It is now so thoroughly a part of the organization of the Kings County Asylum, that one would hardly know that restraint apparatus had ever been used there.

It is unnecessary at this time to again speak of the advantages and method of carrying it out, but we will inquire what influence its adoption and success at the Kings County Asylum has had on the management of other asylums in America. Has it influenced other asylums to favor and adopt this method?

This can best be shown by quoting briefly from recent reports of some of the superintendents.

Dr. P. Bryce, Medical Superintendent of the State Insane Hospital at Tuscaloosa, Alabama, in his report ending September 30, 1882, says on this subject:

"In no department of sanitary science or social science has more progress been made than in the treatment and care of the insane, and it has been our aim to keep as well up as possible with this progress in the general management of our institutions.

"In the treatment of our patients we have *almost entirely succeeded in discarding mechanical restraint* of every kind; and our intercourse with them is characterized by the utmost kindness, candor, and courtesy. There are no leather mittens, muffs, bed-straps, restraining chairs, shower baths, or other terrifying apparatus in use in this hospital.

"In passing through our hospital, especially among the most excitable class of insane, visitors are surprised to witness so much order, quiet, and contentment on every side. They find the inmates well dressed, courteous, and often cheerful in their demeanor.

"We believe it quite possible that this principle of non-restraint may be carried so far as to be injurious in individual cases. We have often refused to allow a patient to be restrained when we knew that a little coercion would do him good.

"The great objection to the use of mechanical apparatus for coercing a refractory patient is its liability to be abused. It is easier, safer, and far better, on the whole, whatever may be the advantages lost in a few individual cases, to discard it entirely when it can be done."

Dr. A. B. Richardson, of the Athens Asylum (Ohio), for the Insane, in his report for 1882, in a number of published tables recorded no instance of restraint, and he says: "The amount of mechanical restraint used during the year has been almost absolutely nothing."

In the State Hospital for the Insane at Norristown, Penn., in his report of 1882, Dr. Robert Chase, the superintendent of the department for men, says: "I am convinced that employment and non-restraint are twin sisters that must go hand in hand in the upward march of improvement in the treatment of the insane."

In the report of Dr. Alice Bennett, the superintendent of the department for women, she says: "No mechanical restraint has been made use of during the year." Dr. Bennett is a strong advocate of non-restraint, as shown by several published articles.

Dr. Dewey, the Medical Superintendent of the Eastern Hospital for the Insane at Kankakee, Illinois, in his report for 1882, says: "The restraint on the male side has been very much less than in the last biennial period; with nearly double the number of patients there were only nineteen instances of restraint for a total of one hundred and twenty-five hours. The restraint during the last year of the period was much lessened (only six occasions); and during eleven months only one instance, with a daily average of over two hundred patients. I have also to report that during the last five months there has been no case of restraint."

Dr. Goldsmith, the Medical Superintendent of the State Asylum at Danvers, Mass., in his report for 1882, says that the use of restraint apparatus in that asylum is a little less than half of one per cent. of the average population.

In the examinations of the Senate Committee of this State, appointed to investigate the asylums, in their session held in New York in the autumn of 1880, Dr. Carlos McDonald, of the Asylum for Insane Criminals at Auburn, N. Y., who then argued in favor of restraint, is, in 1884, carrying out the non-restraint system in his asylum. In his report for 1882, in a foot-note, he says: "No form of mechanical restraint has been used since March, 1882," and in his report for 1883, gives considerable space to its consideration and the results with this method in his management.

The above quotations from the reports of superintendents in various parts of the United States, who are all well known and conducting large institutions of their respective States, show clearly the progress which has been made in this direction by the thoughtful and progressive superintendents.

To learn more fully what progress had been made in this State (N. Y.) I appealed to the State Commissioner in Lunacy, who has kindly furnished me with some information taken from his last report to the Legislature (not yet published). He says:

"During the year the subject of restraint has been specially studied in all of the institutions for the insane. In the poorhouses in which the disturbed insane were found, restraint, in all of its forms hitherto known, is the ordinary method of control. This treatment of the disturbed class in poorhouses seems to be inevitable, owing to the want of facilities for isolating them under conditions favorable to their care, and of proper attendants. In the small county asylums there has been a marked diminution in the amount of restraint, and in several it has been discontinued altogether. This favorable change, it is noticed, has occurred simultaneously with improvements in the accommodations for the insane, and in the employment of more and better attendants. In the larger county asylums less restraint is recorded than formerly; the kinds employed are comparatively mild, and, as at Ward's Island Asylum, it is placed under rigid supervision. The King's County Asylum is the only one which employs no restraint whatever. In the State asylums the amount of restraint is diminishing, and in the State Asylum for Insane Criminals has not been employed in two years. Even in Willard Asylum, with a population of 1,750 chronic insane, there have been days when but one person was restrained, and in that case the simplest and mildest form was used."

As this article has already exceeded its intended limits, we will conclude by answering the question asked earlier in this paper: What influence its adoption (non-restraint) and success at the Kings County Asylum has had on the management of other asylums in America. Has it influenced other asylums to favor and adopt this method?

The above extracts and information give us the data for answering this question as follows:

It has markedly influenced the management of all the asylums in the United States; it has induced many to try and adopt the method, and in a great many to very decidedly diminish the amount of restraint formerly used. It has been shown that a method of management, which previous to 1880 was considered impracticable, is really easy of adoption, and will some day not far distant become almost universal in this country.

The foregoing is the history of the introduction of the "non-restraint" system of treatment of insanity in this country. It shows that its success at the Kings County Insane Asylum, where it was first used, has been the inspiration and support of its use in similar institutions in the United States. The opposition and ridicule with which its introduction was greeted have happily given place to a spirit, first of partial trial, and then of cordial adoption, by those who witnessed or accepted the testimony of its feasibility and success. JOHN C. SHAW.

NEW BOOKS AND INSTRUMENTS.

A Treatise on Bright's Disease of the Kidneys: Its Pathology, Diagnosis, and Treatment, with Chapters on the Anatomy of the Kidney, Albuminuria, and the Urinary Secretion. By Henry B. Millard, M.D., A.M. New York: William Wood & Co., 1884, pp. 246.

In the multiplicity of books of the present day the reviewer in taking up a new volume feels like apostrophizing it somewhat after this fashion:

"For what purpose are you written? Why do you claim the attention of the reading public? What have you that is worth adding to the common stock of ideas that you should come from a press already groaning with unprecedented fecundity? Are you in existence because your author feels:

> "'Tis pleasant, sure, to see one's name in print.
> A book 's a book although there 's nothing in 't'?

Is this why you come to an editor's table for favorable notice, fresh in your new covers and bright gilt-lettered title?"

If it is a medical work that the reviewer has in hand so much the more sternly does he put his questions, and so much the more clearly must the book prove from its pages that it has valuable facts and discoveries to add to the already existing knowledge—facts and discoveries which, but for its author, would not be known.

Turning to the pages of Dr. Millard's treatise on Bright's disease with these thoughts in mind, we place before the reader that which the writer, beyond the management and compilation of his material, can claim as strictly original.

For his histology he depends mostly on Heitzman. From his own investigations he claims that Heidenhain's rod-like structures of the epithelia which form part of a reticulum, considered by some the living matter proper, and intimately concerned in the process of secretion, give rise in the inflammatory process to a new formation of medullary and pus corpuscles. The flat endothelia, which have been recognized by recent observers as lying between the structureless (hyaline) membrane and the basis of the epithelia, are considerably enlarged in both catarrhal and croupous nephritis; that they line the uninary tubules after the epithelia have been shed or lost, and that in croupous nephritis they surround the cast after the epithelia have perished in the formation of the cast.

After the introductory chapters on anatomy and histology of the kidneys, on albuminuria, including a discussion of its microscopic and chemical tests, the author passes on to deal with his subject, Bright's disease, which he considers under the three heads of croupous, interstitial, and suppurative nephritis.

Here the reviewer finds little in his diligent search for new ideas. In the etiology of acute croupous nephritis Dr. Millard illustrates by a case the fact which he thinks has not hitherto been mentioned, that essence of ginger will cause renal inflammation. He also brings forward two cases to support the view that malarial poisoning, though an infrequent factor, may produce acute nephritis.

The treatment is, after all, of the most importance to the practitioner, and the part of the book to which he will first turn. He will find the list of well-known remedies and abstracts of papers in the journals relating to the more recent ones which have been advocated, such as nitro-glycerine, helonias dioica, apis nullifica, euönymus atropurpureus. The author himself finds the chlorides of mercury the most efficacious. He uses the mild chloride in interstitial nephritis, and the corrosive sublimate in croupous. Of the mild chloride he combines one part with ninety-nine parts of the sugar of milk, which he gives in five- to ten-grain doses every two or three hours. Of the corrosive sublimate he prescribes $\frac{1}{10000}$ of a grain every two or three hours. Besides this, he administers three to six drops of a ten-per-cent. mixture of nitric acid three times a day.

The writer is chary of the metric system. He does not neglect it entirely, for he indulges in millimetres now and then; but for the most part he expresses his dimensions in inches and fractions of inches and never deals in grammes or centigrammes. [G. P.]

Medical Diagnosis. A Manual of Clinical Methods. By J. GRAHAM BROWN, M.D., Fellow of the Royal College of Physicians of Edinburgh, etc. Second edition. Edinburgh: Bell & Bradfute. London: Simpkin, Marshall, & Co., 1883, pp. 370.

It is doubtful if more information on the subject of medical diagnosis could be crowded into a similar number of pages. All the means now employed for this purpose are briefly and concisely described, including inspection, palpation, auscultation, microscopical and chemical tests, the use of the sphygmograph, cardiograph, hæmacytometer, laryngoscope, and ophthalmoscope. It is not the book itself, but the tendency of such books, that one would criticise, which is to encourage a superficial knowledge of methods which should be more widely and deeply studied. Its value is similar to that of a small dictionary—useful for reference when the unabridged dictionary or the encyclopædia cannot be had.

[G. P.]

De la Démence Mélancholique. Par le Dr. A. MAIRET, Professeur agrégé de la Faculté de Médecine de Montpellier. Paris: G. Masson, 1883.

The author attempts the separation of certain abnormal cases of general paralysis from the general category, and their erection into a separate class. He justifies this attempt by a critical study of symptoms presented by his cases and a few from other authors, by their pathological anatomy, and by some experiments on dogs.

The semeiology of dementia melancholica may be summed up as follows: (1) a delirium of melancholic type, with ideas of persecution, of hypochondriasis and suicide, illusions and hallucinations—more especially of hearing,—often agitation and stupor; (2) early dementia, as evidenced by silly delusions, apathy, weakness of memory; (3) certain physical symptoms, loss of muscular tonus of facial muscles, slight paresis of face or of upper lid, a muddy complexion with venous stasis near malar region, epileptic and apoplectiform seizures. Speech is often a little thick, but not vibratory as in general paralysis, and inequality of the pupils occurs; no exalted delusions appear, and the melancholic delirium is not systematized. Post-mortem examination reveals thickening of the pia and adhesions to cortex (decortication), exactly as in general paralysis, but limited to the base of the brain, affecting more particularly the orbital gyri, the ventral portion of the fissures of Sylvius, and the temporal lobes in their medio-frontal aspects (gyrus hippocampi especially).

Dr. Mairet considers that the peculiar grouping of symptoms is due not to the nature of the lesion but to its localization at the base. He produced leptomeningitis and superficial cerebritis of the corresponding region in dogs by irritating injections, and noted in them a melancholic, stupid state, independent of any paralysis. In this connection the author does not, it seems to us, lay enough stress upon the coincidence of aural hallucinations, so frequent and strong in his cases, and the lesion of the temporal apex. These facts are strongly supported by Munk's experiments on the auditory cortical areas.

From a study of dementia melancholica, and from his experiments, the author would advance a new cerebral localization—that of depressed, hypochondriacal, and suicidal ideas in the basal and temporal gyri;—a bold suggestion.

In dementia melancholica there is only a slight liability to extension of the lesion to the convexity, and the consequent change of symptoms to those of ordinary general paralysis; in most cases the lesion remains localized at the base.

The prognosis and therapy are as unsatisfactory in this as in other varieties of organic dementia.

[E. C. S.]

Transactions of the American Gynecological Society. Vol. 7, 1882, pp. 485. Philadelphia: H. C. Lea's Son & Co.

This volume is a very handsome memorial of the seventh annual meeting held by the Society in Boston on September 20, 21, 22, 1881. Like its predecessors, it is an excellent piece of book-making in all that concerns the publishers—type, paper, margins, and binding,—and in this respect it offers a pleasing contrast to the bad and dear books American medical publishers are wont to palm off on a docile profession. But the same praise cannot be awarded the editors. No explanation is offered for the great tardiness of publication, and, considering the fact that all the papers, by a by-law of the Society, are required to be handed to the Secretary within two weeks of the time of meeting, and the stenographic reports of the discussions, which were promptly sent to the individual members, had to be returned inside of ten days, there is great remissness somewhere.

Many of the monographs have been privately circulated for months, and the interest of others largely forestalled by other publications in the long interim between the time of meeting and the appearance of these proceedings. Similarly, the admirable

Index of Gynecological Literature is only for 1881, whereas the Index Medicus, from which it is transcribed, has just completed 1883. Lastly, certain of the papers are printed in a different order from that in which they were read, and though a small matter, as no note of explanation is offered, we call attention to it to condemn it.

Turning to the scientific papers, these number fifteen, including those by the two new Fellows, Dr. M. D. Mann, of Buffalo, and Dr. W. H. Baker, of Boston.

The first paper is the address of the President, Dr. Thomas Addis Emmet, of New York.

I.—Dr. Emmet departed from the usual presidential custom in presenting a strictly scientific paper on "A New Method of Exploration, with the Pathology and Treatment of Certain Lesions of the Female Urethra." The new method consists in the following operation, to be done under ether:

The patient being in the Sims position, a specially devised pair of scissors, like those used by seamstresses in cutting buttonholes, is used; the rounded blade in the urethra, the cutting one in the vagina, and closed in the median line, making an incision extending from a quarter of an inch from the urethral outlet nearly to the neck of the bladder. This may also be done with ordinary scissors and tenacula, a grooved staff made of block-tin being introduced into the urethra to keep the parts on the stretch. Should this operation have been done simply for purposes of diagnosis, the opening thus made can be closed by interrupted silver sutures, properly introduced, and allowed to heal. This will necessitate the patient remaining in bed for a week or more. If the operation is preliminary to further treatment, the urethral and vaginal mucous membranes must be stitched together and an urethro-vaginal fistula formed. On the completion of treatment, a second operation under ether will be required to close this artificial opening, much in the same way as is done for closure of a vesico-vaginal fistula. Dr. Emmet claims for this procedure that it is easy of performance, that it is superior to all other methods for purposes alike of diagnosis and treatment of all diseases of the female urethra. He narrates cases of new growths, urethrocele, prolapse of the mucous and submucous tissues, laceration of the urethra from over-dilatation and from child-bearing, which he has cured by this button-hole method. He further states it as his opinion that it offers the best means of diagnosticating fissures of the neck of the bladder and treating urethritis. In the discussion, Dr. Skene agreed that in the treatment of new growths and urethrocele this method was likely to prove of value, but claimed that it was inferior to the endoscope for purposes of diagnosis.

It is too soon to say just how much value this procedure has, until we have the experience recorded by other surgeons who have tried it, but it may be safely affirmed that it is by no means so easy or so safe as its originator would have us believe. The manner in which it is brought forward as a surgical panacea for all the diseases of the female urethra, cannot but militate against its general acceptance by the profession. Just as the more widely known and more valuable operation for restoration of a lacerated cervix, which we owe to the genius of Dr. Emmet, is in a certain disrepute, notably in England, because of the wild claims that have been made for it as a cure-all for uterine disease.

II.—"The Proper Use of Ergot in Obstetrics," by Dr. Joseph Taber Johnson, calls attention to the dangers attendant on the use of this drug, and the evils both to mother and child which result from its present indiscriminate employment. The conclusions the author reaches are:

1. It should *never* be given to a primipara.

2. It should not be given until after labor is completed, or if given prior to that, only when such dilatation is present as to permit of immediate instrumental delivery.

3. In placenta prævia, hemorrhage, and abortion, it should not be given until the uterine contents have been expelled or removed.

The one cardinal indication for its use he finds in atony and relaxation of the muscular walls of the empty uterus, leading to hemorrhage. The author bases these conclusions on the facts: that ergot produces a tonic and unyielding contraction of the muscular tissue of the uterus; and that this contraction may begin at the cervix, and so imprison instead of expel the contents of the cavity, leading to possible death of the child and rupture of the uterus. In the discussion the members concurred in the main with these views, which are so much at variance with much of the teaching of the schools and text-books that they deserve widespread attention. A striking omission, seems to us, in the fact that no one called attention to the question of *dosage.*

III.—"The Ovarian Cell, its Origin and Characteristics," by Dr. Thomas M. Drysdale, is a reply to a paper on the same subject by Dr. Garrigues, read at the previous meeting. The author certainly succeeds in demolishing his antagonist's position pretty thoroughly, but that his successs is equally great in establishing the scientific basis of his own, we are not competent to say. The sympathy of all right-feeling men will be with Dr. Drysdale in the disreputable trick which he was made the victim of by another member of the Society.

IV.—"Some Remarks on Ovariotomy with Special Reference to the Treatment of the Pedicle," by Dr. R. Stansbury Sutton.

This paper contains an historical sketch of the subject, together with the personal observations of the author of the practice of eminent ovariotomists in Europe. Much of the interest of this paper is forestalled by the chapter on ovariotomy in the recent edition of Lawson Tait's book.

V.—"Leucorrhœa Considered in Relation to its Constitutional Causes and Treatment," by Dr. Fordyce Barker. This paper calls attention to a most important and much neglected department of the subject.

The author quotes cases in support of his well-known views, that often, when local treatment has proved unavailing, the patient may be cured by appropriate constitutional regimen and improved morale. He justly urges that the symptom, leucorrhœa, may be the local expression of a constitutional condition; that it may be brought on by new modes of life involving increased nerve wear and tear; and that some of the local pathological conditions which receive such unremitting attention at the hands of enthusiastic gynecologists, are often the effects, and not the cause, of the leucorrhœa.

In conclusion, he points out that this side of the question has been unduly neglected, since the impetus given to local methods of treatment by the brilliant discoveries of Marion Sims.

VI.—"The Care of the Perineum," by Dr. Theophilus Parvin, is a short paper advocating support of that structure during labor, and, if need be, the employment of lateral incisions, to prevent too extensive laceration. The doctor quotes largely in support of his views, and is evidently somewhat burdened by his weight of learning.

VII.—"The Relative Value of Hysterectomy, and of the Complete Removal of the Uterine Appendages for the Cure of Uterine Fibroids," by J. Knowsley Thornton, M.B., C.M. (London). This article gives the statistics—especially of the London surgeons—of these two operations. The author's conclusion, that the latter is much the safer procedure, is corroborated by the recent work of Lawson Tait; but a much larger series of cases will be required before any numerical expression of this result can be arrived at. In the discussion, Dr. Thomas very justly pointed out that many tumors of the uterine wall can be removed by avulsion and enucleation—per vaginam—and that, when practicable, this operation was much safer than either of the ones discussed by Mr. Thornton. The whole question needs further study, especially with reference to the fundamental fact, quietly *assumed* in this paper—does removal of the uterine appendages always cure fibroids and fibrocysts of the uterus ?

VIII.—" The History of Twenty-one Cases of Extra-Uterine Pregnancy Coming under Personal Observation of the Writer," by Dr. T. Gaillard Thomas.

These cases are of great interest, and the conclusions drawn, carry the weight derived from probably the largest individual experience, in the country, of the subject.

Most of the cases have been published before by the author and others, but their collection is of value. The results in the twenty-one cases were :

(1) Six not operated on, two recoveries.
(2) Seven operated on, four recoveries.
(3) Two tapped—from vagina,—both died.
(4) Six treated by electricity, all recovered.

The author's conclusion, that electricity is *the* method above all, is amply warranted and is borne out by the testimony furnished by others, especially Dr. Rockwell, in his recent admirable paper. We are not so sure that Dr. Thomas' restriction of this method to cases prior to the end of the fourth month of pregnancy will hold good. In the discussion, Dr. Goodell reported thirteen cases he had seen. Of these :

(1) Ten were operated on, three recoveries.
(2) Three were not operated on, all died. Truly, not a brilliant record for laparotomy !

IX.—" Electricity in Extra-Uterine Pregnancy," by Dr. Henry J. Garrigues, is a compilation of eleven cases, four being identical with four in Dr. Thomas' series, the only new one—of Dr. Garrigues' himself—must be regarded as of somewhat doubtful authenticity, because the diagnosis was not confirmed by any one else.

X.—" The Influence of the Constant Use of High-heeled French Shoes upon the Health and Form of the Female, and upon the Relations of the Pelvic Organs," by Dr. Samuel C. Busey. This paper is charmingly written, and seems eminently sound from the orthopedist's point of view ; but from the gynecological stand-point the author's deductions, while they seem reasonable, must be regarded—until definite evidence is adduced—as " not proven."

XI.—" The Mechanical Therapeutics of Versions and Flexions of the Uterus," by Dr. Ely Van de Warker. This is a most able analytical study of a most difficult and obscure subject. The author begins by stating two problems, which he tries to solve in this paper :

1. " What are the limits imposed by the uterus and its appendages upon the mechanical agencies acting upon it ?"—in other words, what should we expect *not* to do by the use of a pessary ?

2. " The action of the mechanical forces under these limitations," or *how* do the pessaries in common use accomplish their results ?

Under the first of these headings he proceeds to show that (*a*) a certain amount of uterine mobility must be allowed ; (*b*) that the vagina imposes definite mechanical limits to the action of any pessary ; (*c*) that pessaries should not injure the soft parts ; (*d*) nor interfere with the function of any of the pelvic contents.

Under the second heading he classifies all the more commonly employed pessaries—over one hundred are figured,—and makes three groups :

1. Pessaries combined with external support.
2. Pessaries acting wholly intra-vaginally.
3. Pessaries acting within the body of the uterus—intra-uterine stems.

Each of these groups is subdivided, and the author then takes up each subdivision separately and the individual pessaries belong to it. While reserving his final conclusions for a subsequent paper, the author shows in his incidental comments such a clear, critical faculty that we shall eagerly look forward to his next contribution to this subject.

His general condemnation of group one, in most cases, and that portion of group three which calls for an external support, in all cases, seems eminently sound. No less admirable, we think, is his analysis of the second group ; but it will be much less readily assented to, especially his demonstration of the pulley—not the lever—action of a Hodge retroversion pessary. Any one who will master the premises so fully and logically here set forth will be in a position to estimate, at their value, the somewhat pessimistic remarks recently made by Dr. Matthews Duncan on the subject of pessaries.

XII.—"A New Method of Operation for the Relief of Rupture of the Perineum thro' the Sphincter and Rectum," by Dr. J. Collins Warren. The author proposes to dissect up the perineal flaps before uniting them by sutures, and claims to lessen the tension on the recto-vaginal septum, and thereby get a better anal orifice.

XIII.—"Measurements of the Uterine Cavity in Childbed," by Dr. Wm. L. Richardson, gives a table of two hundred and sixteen cases.

XIV.—"Surgical Operations on the Pelvic Organs of Pregnant Women," by Dr. M. D. Mann, is an interesting collection of cases, many of them new. The author's conclusions are that these operations are much less dangerous than are generally supposed—those on the rectum excepted. More clinical facts are needed before any very positive propositions can be safely formulated.

XV.—"Hyperæmia of the Vesico-Urethral Membrane," by Dr. W. H. Baker, is a most painstaking contribution to a little-understood subject. He reports five cases all cured by the establishment of a temporary vesico-vaginal fistula, after the diagnosis had been made by means of Skene's endoscope. The high value placed by the author on this means of diagnosis is in marked contrast to the views enunciated by Dr. Emmet in his address, and we presume that Dr. Emmet would have used his "button-hole method," if these cases had come under his care.

A graceful tribute to the memory of the late Professor James Platt White, of Buffalo, by Dr. Thomas, concludes the volume.

Looking at this volume as a whole, it must be confessed that it is somewhat disappointing. The great reputations enjoyed by many of the members of this Society would naturally lead us to look to its Proceedings for their best efforts—their most recent as well as their weightiest opinions. But in this we are not gratified; the papers, with a few exceptions, are not up to the standard such a Society might attain, while the discussions fall distinctly below what we had a right to expect in critical value.

[W. M. T.]

Diseases of the Brain and Spinal Cord. By DAVID DRUMMOND, M.A., M.D. London: Henry Kimpton, 1883, 8vo, pp. 374.

In so short a treatise, printed in coarse type, Dr. Drummond has attempted to present a summary of our knowledge of the diseases of the central nervous system, and, considering the extremely limited space at his command, he has been fairly successful. The busy practitioner or student who desires to seize the salient features of these affections can here do so in a few minutes. Still it must be said that the knowledge thus acquired will be only fragmentary and provokingly abbreviated. This defect, that of over-condensation, is more evident in sections on "diagnosis," and in treatment. As a rule, the morbid anatomy and symptoms of the various affections are clearly and sufficiently stated.

The work is divided into two parts: one on the brain, the other on the spinal cord; each part opening with a fairly good account of the anatomy and physiology of the organs. With respect to the brain it may be noted that its vascular supply (areas) and cortical centres receive due attention. In the account of the cerebral convolutions, the paracentral lobule is omitted. The cerebellum is designated as "perhaps the most important organ of co-ordination in the brain," a statement which extirpation experiments in animals scarcely warrant. Nearly four pages are devoted to the electrical reaction of paralyzed muscles, and the altered "formulæ" are well given, but it seems to us that the author does not lay enough stress upon the slow, wave-like contractions so characteristic of the reaction of degeneration. The account of convulsive attacks is very good. Choked disc as a sign of brain disease is well described, the author rightly saying that the use of the ophthalmoscope "is a *sine qua non* in nervous diseases." We cannot, however, endorse his statement that "the pupil should be well dilated with atropine before a positive opinion is formed as to the condition of the disc and retinal vessels." This procedure, so annoying to the patient, is, we believe, needed in only a small number of cases, as with the upright image these parts can be well seen through a medium or slightly dilated pupil.

The article on cerebral hemorrhage is marred by the use of the term "apoplexy" as a synonym, thus diverting it from its proper use of designating a symptom-group irrespective of its pathology. The section on embolism and thrombosis is also good; but a couple of temperature charts would have much assisted the student for differential diagnosis. The subjects of hyperæmia and anæmia of the brain are treated in a moderate and conservative manner, in contrast to the exaggeration of the frequency of these affections which mars some other works on nervous diseases. We can endorse the author's statement that "anæmia of the brain is one of the commonest causes of headache." The hydrocephaloid state of Marshall Hall is described; and the statement made that choked disc may be looked for in severe cerebral anæmia. The author believes that the state of the cerebral circulation is indicated or reflected by that of the retina—a statement which not all observers will accept. Other good articles are those on cerebral syphilis (the author under-estimates the frequency of cerebral lesions in the first year of the infection), tumors, and general paralysis of the insane, though in the summary of the pathological anatomy of the last no mention is made of the most striking morbid histological appearance, that of the so-called spider-cells throughout the cerebral mass. Dr. Drummond does not believe that any case of general paralysis has ever been cured ; and he evidently inclines to the view that syphilis is one of the factors of the production of this terrible disease. Chronic abscess of the brain is pretty fully treated and due stress laid on the extraordinary remissions which occasionally interrupt the fatal course of this disease. In the treatment of cerebral syphilis, nothing is said of the necessity of giving large doses of iodide of potassium. Dr. Drummond has met with "mental disturbances" much more frequently in cases of intracranial tumor than we have, and he apparently exaggerates the frequency of choked discs in this affection. Three interesting cases are briefly narrated in illustration of this section. Hyoscyamine is recommended in the treatment of general paralysis during its stage of excitement, and ⅛ grain is given as a safe initial dose—but the student is not warned that this does not refer to the crystallized hyoscyamine which is six or eight times stronger than the amorphous. Epilepsy has a considerable section devoted to it, but chorea, hysteria, tetanus, and hydrophobia are left out, though they are very probably connected with functional or organic changes in the central nervous system.

The structure and functions of the spinal cord are fairly well though briefly set forth, and the various diseases of this organ are described in a little over one hundred pages. The account of ataxia is good ; the author thinks Erb's statistical statement of syphilis as a cause of the disease is extreme ; he believes that most cases are caused by cold and exposure acting upon a depressed nervous system.

The wood-cuts, especially the anatomical ones, are very rough and even indistinct.

On the whole, it seems to us that this attempt at conciseness is unsatisfactory because of lack of details, especially with reference to treatment ; we have certainly passed the time when the pathology of the most important department of medicine can be so condensed. [E. C. S.]

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ARCHIVES OF MEDICINE.

Original Articles.

A PRELIMINARY PAPER UPON THE CAUSES OF LATERAL CURVATURE OF THE SPINE; WITH THE REPORT OF A CASE OF LATERAL DEFORMITY DUE TO OCCUPATION.

By NEWTON M. SHAFFER, M.D.,

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THE great variety of opinions and theories that exist regarding the etiology and pathology of lateral curvature of the spine, with and without rotation, leads me to place upon record the following case. It presents many interesting points, and illustrates a form of curvature which, I think, has not been fully described, although the causes which produce it, in this individual case, are those most frequently cited by various eminent writers on the subject of scoliosis.

Lateral curvature of the spine has been written upon by so many eminent men, and the subject has been so frequently reviewed, that it may be well, before recording the case that will form the basis of my remarks, to state that I have nowhere seen that lateral curvature has been subjected to the close scrutiny which many other conditions have undergone. I regard this fact as indicative of the lax way in which all strictly objective conditions are regarded. If true scoliosis were accompanied by pain or by extensive destruction of tissue, or by any serious complication, I feel that the causes producing it would have received the attention which they deserve. It is only fifty years ago, for example, that club-foot escaped from the realm of the charlatan to the domain of scientific surgery. Within a few years only has knock-knee received the surgical as well as the mechanical attention which it requires, and, although true scoliosis received much attention several years ago, and especially from the French Academy, and many muscles were divided in the attempt to make myotomy answer, in this condition, the purpose of tenotomy in club-foot, the attempt was long since abandoned. True scoliosis, therefore, has little to recommend it to the general or operative surgeon. The requirements of its treatment are almost wholly mechanical, and the most intricate problems in its etiology and pathology still remain unsolved.

Therefore, in dealing with this subject wherein there is so much uncertainty regarding the pathology, and the etiology of which is so obscure, I have thought it well to study the matter from a clinical standpoint until such time as actual post-mortem exploration shall settle certain vexed questions, and, in so doing, I have been able to clearly define eight different varieties of lateral curvature of the spine, each with a distinct etiology, and with easily demonstrated clinical features that should enable the careful observer to differentiate between the several forms.

The varieties are as follows:

1. The true scoliosis, with rotation of the vertebræ.
2. The hysterical imitation of the true curve.
3. The incidental curve due to an unequal length of the lower extremities, etc.
4. The curve due to unilateral lung or pleural disease—first described by Dr. Little, of London.[1]

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[1] "On Spinal Weakness and Spinal Curvatures." By W. J. Little, M.D. Longmans, Green, & Co., London, 1868.

5. The curve, with exaggerated rotation, due to infantile paralysis—first described, I believe, by the writer.[1]

6. The lateral curve, which accompanies the first stage of Pott's disease in the lower and especially the last lumbar vertebræ. To this may be added the lateral deviation that may occur in any case of chronic spondylitis.

7. The rachitic curve; and

8. The curve due to an atonic condition of the fibrous and muscular tissues in the adolescent when prolonged mal-position is maintained, as a matter of occupation especially.

I have had ample opportunities for observation in all of these conditions except the last-named, and the object of this paper is to place upon record a case of lateral curvature due to weakened tissues and habitual mal-position, and to point out the clinical features that characterize that condition.

John L., aged seventeen, born in Ireland and residing in New York, presented for examination at the N. Y. Orthopædic Dispensary on Feb. 4, 1884. The hereditary history shows that his father died of phthisis; his mother is living, and seemed, when the patient was examined, in good health. One sister of the patient has hip-joint disease, one brother is now suffering from inflammatory rheumatism; two brothers are dead from causes unknown. No other facts bearing on heredity could be ascertained.

The patient is a very poorly nourished lad; he has a slight amount of ectropion; his lips are large, thick, and almost colorless; and his nutrition is generally bad. Two years ago, the patient, then fifteen, and apparently straight, entered the service of a grocer in this city as an errand or delivery boy. His employer did not use a horse and wagon for delivery, and all goods delivered were carried by this boy in a basket. The work was laborious and the hours long. In order to facilitate the carrying of the numerous parcels entrusted to him, many of which were very heavy, the patient habitually threw his body forward and to one side, hooking his right arm through the handle of his basket, and supported his burden upon the right ilium ; he was in this position a good deal of the time, and, after working steadily for about seventeen months, he began to experience pain in the "right hip" (iliac region), and his friends noticed that he was becoming crooked. As his wages, however, were the chief dependence of his mother's family, he continued his work, disregarding the pain and the increasing mal-position. The pain began to increase, and to trouble him somewhat when he arose in the morning, or upon changing his posture after a period of rest ; he found that he could not sleep upon his right side on account of the pain so caused ; he began to limp, "favoring" the right leg, and as the pain and mal-position increased, he was at length obliged to abandon his work and to seek medical advice.

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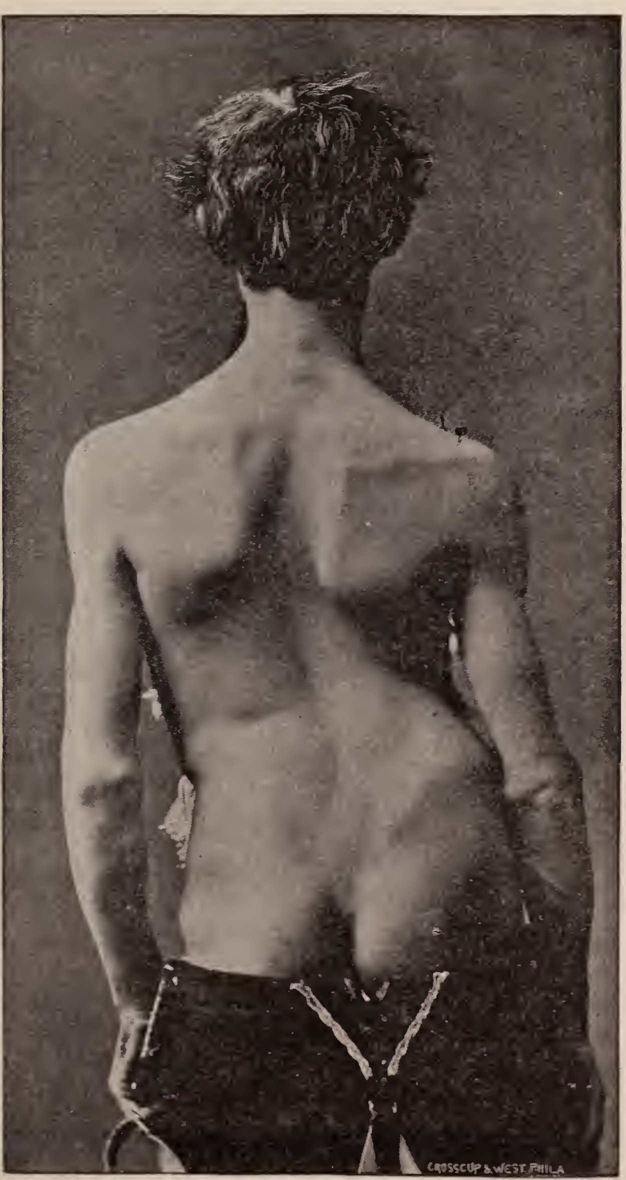
[1] "Pott's Disease, its Pathology and Mechanical Treatment, with Remarks on Rotary Lateral Curvature." By Newton M. Shaffer, M.D. G. P. Putnam's Sons, N. Y., 1879; also, a Lecture on Lateral Curvature of the Spine. By the same author. *N. Y. Medical Gazette*, April 2, 1881.

He first consulted a physician who thought he might have hip-joint disease and referred him to Dr. I. Adler, who eliminated hip disease and sent the patient to the Dispensary. Upon examination, it was found that the pain was referred to several places: 1st, in the back (dorso-lumbar region) ; 2dly, to the right ilium and especially at the crest, posteriorly, where he had borne his burdens ; also in the region of the hip ; and 3dly, along the course of the sciatic nerve on the right side. The pain was always "in the back" simply, or "in the hip." None of our inquiries gave rise to any more exact or explicit statement. There were no points of tenderness along the spine ; pressure giving negative results. There was no evidence of chronic spondylitis, sacro-iliac disease, or morbus coxarius. The spine was normally flexible antero-posteriorly. There was no marked lateral resistance on either side in the dorsal region, and a pronounced lateral curvature (see engraving) with a very marked prominence of the right ilium, posteriorly. The last was a marked feature of the case. On a casual examination it seemed almost as if there were a bony growth at the posterior iliac spine—which impression was strengthened, if any thing, by comparison and palpation.

After repeating the tests above mentioned, with the same results I directed the patient to lie down in the prone position. The iliac prominence was then greatly modified, and the spinal curvature much reduced. Gentle traction was then made, one assistant pulling at the axillæ and another at the ankles. After a moment's traction the curvature practically disappeared and the iliac prominence was very greatly reduced. There was no undue rigidity of the spine, as already mentioned, and, as stated, the application of gentle force sufficed to remove all the essential features of deformity.



The patient was then tested as to the peculiarities of the lateral curvature of the spine. The lower extremities were of equal length and this element of mechanical etiology was eliminated. The curve in the lower lumbar region was very short and slightly to the right; it then passed by a long sweep to the left, forming a compensatory curve in the dorsal region. The right shoulder was depressed and there was a slight difference in the scapulæ, the left being somewhat higher. There was a very slight degree of rotation of the dorsal vertebræ toward the left side. Putting the patient to the test of extreme anterior flexion (Adams' test) it became almost straight, and there was then noticed a slight rotation at the lower (lumbar) curve, toward the right side. When the patient was placed in the prone position and traction made, the dorsal and lumbar rotation disappeared. None of these tests gave pain, and there was no evidence of reflex muscular spasm, such as is seen, for example, in chronic spondylitis or in hip-joint disease. Gravity as an etiological factor being removed, the spine, on slight traction, returned in a very brief time to a nearly normal position. All the prominent features of the deformity recurred when the patient reassumed the erect posture. The patellar tendon-reflex was slightly exaggerated on both sides, as were also the other reflexes below the diaphragm. The curve had existed for seven months, during most of which time the patient, with no improvement as to his condition and without modifying his efforts, had continued to work as described above. He was finally compelled to desist by pain and debility, and not by the increasing deformity.

Had this lad been engaged in an occupation that necessitated prolonged standing, instead of walking with his spine distorted, he would probably have developed adolescent knock-knee or flat-foot,—so frequently seen among boys of his age. Be that as it may, he came to us with a deformed spine which presented the features described, and which we have attempted to show in the engraving.

Among the many causes that have been assigned for lateral curvature—the true scoliosis of growing girls with rotation—there is none that is oftener dwelt upon, perhaps, than habitual mal-position. The patient, for example, has been observed to stand habitually, with the weight of the body supported by one limb; or the posture of the subject at the school-desk has been observed as awkward, or somewhat out of normal; and these or similar causes have been accepted as the important etiological factors in true scoliosis. My friend, Dr. W. J. Little, of London, has very recently stated his views upon this subject, and, as they may be accepted as representing a large class of readers and thinkers, it may be well to state them here. Dr. Little says, in his excellent and exhaustive treatise on "The Medical and Surgical Aspects of In-knee (Genu Valgum[1])," (page 18,) speaking of knock-knee: "In adolescents, it is probable that carrying heavy weights, fatigue, and long hours of work have their principal share in its production, favored in fast-growing lads by insufficient diet and consequent weakness of tissue." Again, page 99, he says: "Nature or gravity works in the lower extremities exactly as in the spinal colum afflicted with scoliosis or twisting of the spine, which, we have already stated, is a strictly analogous distortion to in-knee, occurring in distinct forms,—the atonic and rachitic."

The case we have placed upon record would sustain these views, if the history so recorded were the history obtained in true scoliosis with rotation. The present is, however, a very exceptional case in our experience, but, so far as it goes, it corroborates the views of Dr. Little, as above quoted, and no one will deny, I think, that the conditions named by him may produce lateral curvature. Instead of resulting in true scoliosis, however, we have the condition, aptly called by Dr. Ketch, of the Dispensary, "a grocer boy's back."

My friend, Dr. Seguin, has also recently verbally described to me a condition which may be called an "office boy's back," wherein the deformity resulted from carrying bank or other heavy ledgers, etc., and, while it would be impracticable in this paper to point out the differences between this curvature and the other curves, to which we have alluded in our classification, it might be well to state briefly the chief differences betweeen the "grocer boy's back," and that found, for example, in growing girls with rotation.

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[1] London: Longmans, Green, & Co., 1882.

In my monograph on Pott's disease already referred to, I call attention to the fact that permanent lateral curvature with rotation does not result from inequality in the length of the lower extremities, after such disabling conditions as morbus coxarius, and infantile paralysis of long standing. I have had, since the monograph was written, opportunities to observe many cases of adolescent girls with suppurative hip-joint disease, wherein the affected limbs have been shortened practically from three to five inches or more. In these cases, the prolonged suppuration produced just that atony of the fibrous and muscular tissues that Dr. Little and others place so much stress upon, and the habitual malposition (also prolonged) was always present when the patients stood or walked. None of these cases have developed any thing more than the incidental curve (the third of my classification). All evidences of curvature disappeared in these patients when they were placed in the prone position. Most of them complained, as did John L., of pain somewhere in the back. It has appeared to the writer, therefore, that even atony with prolonged mal-position is not a very frequent cause of true lateral curvature with rotation. The cases are exceptional, I think, wherein these causes produce a permanent curve, *i. e.*, one not obliterated by recumbency, or, as in the case of John L., by recumbency and slight traction.

And this brings us to speak of the principal features in true lateral curvature with rotation. There are many matters that we cannot touch upon on this occasion, and we will refer only to those involved in this case. The first is the permanency of the curve—and the permanency of the rotation, even if slight in true scoliosis. If we place the patient in the position of extreme anterior flexion, with even a slight curve and slight rotation, instructing the patient to attempt to touch the toes with her fingers without bending the knees, the curve remains the same, or is slightly increased, and the rotation is exaggerated. In the case of John L., the lateral element almost wholly disappeared during this test, as did the dorsal rotation. The rotation in the lumbar region, which was slight, and out of all proportion to the curve, became slightly exaggerated, as was to be expected, as this was the point of true curvature, those above being secondary.

If we place a patient with true scoliosis in the prone position, the antero-posterior curves become modified, as do also the compensatory lateral ones, but the true lateral and rotary elements do not alter even with traction. In other words, we have a structural change in the muscles in true rotary lateral curvature, with, of course, changes in the vertebral bones, which, I think, are secondary. In the case of John L., there is an atonic condition only, without, I believe, structural change in the muscular or fibrous tissues. In true scoliosis there is a permanent, progressive curve, involving especially the *intrinsic* vertebral muscles, while in the case here recorded there seems to be *extrinsic* disturbance only. Ether given to the true scoliotic would fail to modify the curve; if administered to John L. it would have removed the curve at once (inasmuch as recumbency and slight traction did so), though it had existed for seven months or more. In the former condition there is a permanent loss of equilibrium between antagonistic muscles; in the latter the disturbance is almost entirely, if not wholly, functional.

Let me ask those who accept and teach the mechanical etiology of scoliosis, why it is that the curve and twisting occur most frequently in the mid-dorsal region. The thorax forms a pedestal for extensive extrinsic muscular action,—and none of these extrinsic muscles are in a position to produce a thoracic or vertebral deformity, unless their distal extremity becomes a fixed point. Again, there is scarcely any motion at the first five dorsal vertebræ, and the remaining ones—down to the tenth dorsal vertebra—also possess very little mobility. The location of these ten vertebræ, their costal as well as their ligamentous and muscular attachments, the shape of their inter-vertebral discs, their almost interlocking spinous processes, their vertical articular processes, and their situation as regards the centre of gravity (which passes anteriorly to the bodies, in the mid-dorsal region, in the erect position),—all tend to make this the region of the least mobility in the whole spinal column. If the primary curvature were to occur in this region from gravity, would it not be a *posterior* one, as in chronic spondylitis,—this being the normal direction of the spinal curve at this point? And would not the curve occur, if gravity were the factor, at the point of the least resistance, rather than at the point of the greatest resistance—in other words, between the thorax and the pelvis, as it did in the case here recorded? And yet, true scoliosis, with twisting, occurs most frequently in this mid-dorsal region, where there are so many obstacles to its occurrence. To make gravity, with predisposing atonic conditions, accountable for a curve which finds its principal expression at this doubly locked region, seems to the writer wholly untenable.

In the case I have recorded the cause, or causes, are not hard to find. They exactly accord with Dr. Little's graphic description of the cause of adolescent knock-knee. There are many reasons why Dr. Little's etiology, as applied to the development of true scoliosis, should not be accepted. And I have ventured to place the case of John L. upon record as an illustration of this position.

I would like very much to take one step more in this connection, and dwell at length upon what I have come to think are the causes of lateral curvature. I am obliged, however, to desist, both on account of lack of time, and because I am now engaged in making certain tests and in conducting some experiments bearing upon the subject, which are still incomplete. In the lecture already referred to, I have said: "There is, undoubtedly, a localized or central neural lesion, which probably is developed early in life" in this condition. I can only reaffirm this conclusion on this occasion.

IODOFORM IN DIABETES.

A REPORT OF TWO CASES SHOWING COINCIDENT DIMINUTION OF SUGAR AND OF UREA TOGETHER WITH AN INCREASE IN WEIGHT; ALSO AND MORE ESPECIALLY SHOWING THAT TOXIC DOSES OF THE DRUG ARE UNNECESSARY AND, FURTHER, THAT THEY ARE PREJUDICIAL TO ITS THERAPEUTIC ACTION.

By SARA E. POST, M.D.

THE subjects of the following experiment presented the symptoms of diabetes mellitus in a pronounced but moderate degree. Increase in the quantity of urine with an increase of sugar and of urea, together with a commencing diminution of weight, is to be noticed in their history.

Case 1 was a woman of forty-eight years. She had been married for ten years. Previous to her marriage she was healthy. She has been, at different intervals during the past five years an out-patient of the New York Infirmary. No record of her illnesses can be found, but she is said to have been treated for specific disease, and tradition gives to her a profuse pustular eruption about three years ago. At the date of my first observation, she presented no story of an initial lesion, and she had no enlarged glands. Her hair, however, was scanty, and she had frontal redness, swelling, and tenderness with intractable fissures in the angles of the mouth. She was put upon the iodide of potassium, .60 t. i. d. Even at this time she complained principally of sleepiness during the day, with broken and irregular rest at night. May 21st, four weeks later, she had not been relieved of these symptoms and she had added to them a pruritus vulvæ. Examination revealed swollen labia with general eczema of the surrounding parts. Sugar was found in the urine, the odor of which was bad. The iodide was continued with a special reference to the diabetic state. After five days the sugar showed increase rather than diminution. The iodide was stopped, and for the four succeeding days no medicine at all was given. The sugar during these four days continued steadily to mount. By short steps of progression it increased from 151 grms. to 165 grms. for twenty-four hours. At his point, upon June 1st, 1883, the iodoform treatment was commenced.

Case 2, was a woman of sixty years. She had a history of financial reverses and of family anxiety and care. Though upon her presentation at the clinic she still weighed 190 pounds, she said that she had "failed," that previously she had weighed much more. In her case also a vulvar eczema directed attention to the disease. For three weeks previous to the commencement of medication the urine in this case was observed for the determination of her average excretions per day. Her quantities were found to be comparatively constant, the sugar oscillating in the neighborhood of 65 grms. per 24 hours. Iodoform was first given in this case August 13, 1883. Neither of these cases were restricted in diet. Case 1 lived principally on bread and tea, while case 2 had the usual diet of the middle class. In neither case was the appetite voracious, still it was very good. Thirst was not marked.

The details of our experiment upon these patients are to be found in the table which follows. In general terms it may be said that iodoform was given to case 1 from June 1st to June 16th, and again from August 13th to November 5th. Her history, therefore, presents two periods of medication which are separated by two months. To case 2 iodoform was given from August 13th to September 10th; again from October 15th to December 17th, and again from January 7th to January 28th. Her history, therefore, presents three periods of medication, the first separated from the second by one month, and the second separated from the third by three weeks. It may be stated further that the therapeutic influence of the drug was purposely overstepped in its administration.

CASE I. 1st Series. Date.	Quantity of urine in 24 hours.	Specific gravity of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Patient's weight.	Other symptoms.	Additional treatment.
May 22.		1037	34							K. iod.=.60 grm. t.i.d.
" 23.		1033	32							"
" 24.	3750 cc.	1028	28.6	107						"
" 25.	3500 "	1029	32	112						"
" 26.	3250 "	1031	33	107						"
" 27.	4250 "	1031	35	148.7						"
" 28.	4875 "	1031	32	156						
" 29.	4000 "	1034	38	152						
" 31.	4375 "	1031	33	144			1 grm.			
June 1.	5020 "	1031	33	165	10.4	52.2	1 "		Color and odor improved.	
" 2.	3500 "	1029	30	105			1 "			
" 4.	4110 "	1028	32	131			2 "			
" 5.	4420 "	1022	21.7	95.9			2 "			
" 6.	3550 "	1026	26	92.3			1 "			
" 7.	2725 "	1021	23.7	64.5			1 "		Domest quarrel.	
" 8.	2975 "	1025	26	77.3			0.66 "		Diarrhœa.	
" 9.	5250 "	1022	22.7	119			2.0 "		"	
" 10.							2.0 "			
" 11.							2.0 "			
" 12.	4450 "	1018	16.7	75.9			2.0 "			
" 13.	3500 "	1018	18	63			2.0 "			
" 14.	2780 "	1020	20	55.6			2 "			
" 15.	2185 "	1023	26	56.8			1 "		Vomiting & diarrhœa.	
" 16.	2400 "	1014	11	26.4			1 "		Anæmia & pustular eruption.	Tinct. ferri chlor., .30 grm. every 2 hrs.
CASE I. 2d Series. Date.										
July 10.	1900 "	1019	20.8	39.5					Purplish boils over body and limbs in l'ge numbers.	
" 12.	1575 "	1020	21.7	34						
" 14.	5500 "	1016	16.7	91.8						
" 17.		1026	27.7							Tinct. ferri chlor., 1. grm. 3 times a day.
" 19.	4400 "	1032	29.4	129					Morally excited.	"
" 21.	6000 "	1020	17	102					Vomiting.	"
" 24.		1012	10.8						Diarrhœa.	"
" 26.	4100 "	1020	20	82					"	"
" 28.	4500 "	1020	21	94.5					"	"
Aug. 2.		1020	20						"	"
" 6.	3000 "	1026	22.7	68					Feels well	Reduced iron, .30 grm. 3 times a day.
" 13.	2200 "	1030	23	50.6	13	28.6	0.66 "	63 kilos.	"	"
" 14.							0.66 "		"	"

CASE I. 2d Series—cont'd. Date.	Quantity of urine in 24 hours.	Specific gravity of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Patient's weight.	Additional symptoms.	Additional treatment.
Aug. 15.							0.66 "		Feels well	Reduced iron, 0.30 grm. 3 times a day.
" 16.							0.66 "		"	"
" 17.							0.66 "		"	"
" 18.							0.66 "		"	"
" 19.							0.66 "		"	"
" 20.	2550 cc.	1009	9	22.9	9	22.9	0.66 "		"	"
" 27.	2000 "	1015	16	32	11	22	0.66 grm.		"	"
Sept. 4.	2500 "	1020	17	42.5	10	25	0.66 "			"
" 10.		1023	22		12		0.66 "	64 kilos.	Diarrhœa.	"
" 17.	1600 "	1022	21.7	34.7	15	24	1.3 "			"
" 24.	2250 "	1025	23	51.7	12	27	1.3 "		Weak and trembling.	Iron stop'd on account of diarrhœa
Oct. 1.	1440 "	1030	29	41.7					Diarrhœa.	
" 8.	1440 "	1021	19	27.3	14	20		63 kilos.	"	
" 15.	1440 "	1031	29	41.7	13	18.7	1.3 "		Pale and nervous.	
" 22.	1920 "	1022	16	30.7	20.8	39.9	1.3 "			
" 29.	1440 "	1030	33	47.5	13	18.7	1.3 "		"	
Nov. 5.	2500 "	1031	35	87.5	14	35	1.3 "			

CASE II. 1st Series. Date.	Quantity of urine in 24 hours.	Specific gravity of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Albumen in urine	Patient's weight.	Other symptoms.	Additional treatment.
July 12.	2640 cc.	1026	25	66							
" 14.	3120 "	1030	27.7	86.4							
" 17.	3230 "	1024	17.9	57.8							
" 19.	2640 "	1030	27	71							
" 21.	2400 "	1030	27	64.8							
" 24.	2160 "	1030	27	58							
" 26.	1860 "	1030	27	50							
Aug. 2.	2884 "	1028	27	77.8							
" 4.	2400 "	1030	29	69.6							
" 6.	2040 "	1030	29	59							
" 9.	2640 "	1030	29	76.7							
" 11.	2880 "	1029	27	77.7							
							Grms.	Considerable.	Kilos.		
" 13.	2280 "	1037	30	68.4	20	45.6	0.66	Less.	87		0.30 grm. redc'd iron.
" 14.	2520 "	1038	31	77.0	22	55.4	0.66	"			"
" 15.	2400 "	1036	29	69.6	17	40.8	0.69	Trace.			"
" 16.	2880 "	1040	33	95	18	51.8	0.66	"			"
" 17.	2160 "	1035	27	58.3	15	32.4	0.66	"			"
" 18.	3120 "	1032	25	78	13	40.3	0.66	"			"
" 20.	3360 "	1030	22.7	76.2	14	47	0.66	"		Eruc'tions.	"
" 21.	2280 "	1040	31	70.6	24	54.7	0.66	"		Ap'tite po'r	"
" 22.	2280 "	1035	30	68.4	19	43.3	0.66	"		"	"
" 23.	2640 "	1021	23.9	63	14.5	38.2	0.66	"		"	"
" 24.	2160 "	1033	20.8	44.9	25.5	55	0.66	"		"	"
" 25.	2640 "	1020	20.9	55	14	36.9	0.66	"		"	"
" 27.	2520 "	1032	21.8	54.9	18	45.3	0.66	"		"	"
" 28.	2640 "	1020	21.8	57.5	13	34.3	0.66			"	"
" 29.	2280 "	1022	16	36.4	13	29.4	0.66			"	

CASE II. 1st Series —cont.'d Date.	Quantity of urine in 24 hours.	Specific graviy of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Patient's weight.	Other symptoms.	Additional treatment.
Sept. 3.	2160 cc.	1030	28	60.4	20	43	6.66 grm.		Trembl'g.	
" 10.	2040 "	1027	23	46.9	16	32.6	2.0 "	83 kilos.	"	Tinct. ferri chlor., 1. grm. 3 times a day.
" 17.	2760 "	1025	23	63.5	14	38.6			"	"
" 24.	2400 "	1025	33	79	10	24			"	"
Oct. 1	3120 "	1028	27.7	86.4					"	"
" 8.	2640 "	1027	29	76.5	15	39.6				"
CASE II. 2d Series.										
Date.										
Oct. 15.	2280 "	1030	31	70.6	15.5	34.2	1.3 "		Feels well	"
" 22.	2640 "	1025	22	58.0	15	39.6	1.3 "		"	"
" 29.	2160 "	1026	26	56	14	30	1.3 "		"	"
Nov. 5.	1920 "	1026	25	48	15	29.6	1.3 "		"	"
" 12.	2160 "	1028	22	47.5	20	43	1.3 "		"	"
" 19.	1560 "	1025	26	40.5	11	17	1.3 "		Trembl'g.	"
" 26.	2280 "	1020	16	36.4	13	29.6	0.33 "		"	"
Dec. 2.	1560 "	1025	25	39	16	24.9	0.33 "	86 kilos.	"	"
" 10.	1800 "	1029	28	50.4	16	28.8	0.33 "		"	"
" 17.	2160 "	1030	28	60.4	13	28.8	0.33 "		Sleepy & trembling.	"
CASE II. 3d Series.										
Date.										
Jan. 7.	2040 "	1029	29	59	8	16.3			"	"
" 14.	2400 "	1028	27.7	66.4	18	43	1.3 "	84 kilos.	Sleepiness gone.	"
" 17.	1800 "	1024	19	34	6	10.8	1.3 "		Feels well	"
" 21.	1800 "	1021	15	27	9.7	17.4	1.30 "		"	"
" 24.	2040 "	1025	25	51	16	32.6	1.30 "		"	"
" 26.	1800 "	1031	29	52	14	25	0.66 "		Paresis of low'r extremities, lasting 10 minutes, fol'wd by diar'hœa Many of her teeth are loose.	"
" 28.	1800 "	1020	18	32.4	10.7	19.2	0.66 "	82 kilos.		"

For the chemical work which is here represented we are indebted to Miss Chevalier, Lecturer upon Chemistry in the Woman's Medical College of the New York Infirmary.

The weekly variations in these quantities are represented in the chart. This weekly determination was made by averaging the daily results during that period, with the exception that when medication was commenced in the middle of the week, only the results which followed its commencement were employed. The chart presents a view of the tables which is moderate. Both higher and lower points than are there represented were obtained. In general terms, the chart pictures only the persistent effect; the evanescent effects, as well as the spontaneous oscillations of the disease, tend to be excluded from its record.

Following the chart, case 1 will be found with an average of 140 grms. of sugar per twenty-four hours when she commenced the iodoform treatment. A high average had been maintained for at least two weeks previous to this time. One gramme of the medicine was given per day, finely powdered and enclosed in sealed capsules, .33 grm. or 5 grains being taken after each meal. The medication was commenced in the middle of the second week represented upon the chart. The quantity of sugar did not immediately fall; on the contrary, a slight elevation occurred. During the following week, however, it reached the average of 88 grms., and the week after it descended still further to the average of 55 grms. in 24 hours. During this week the dose of iodoform was increased to 2 grms. or to ten grains three times a day. Vomiting and diarrhœa occurred with a pronounced anæmia and a pustular eruption upon the skin. The iodoform was discontinued, the diarrhœa received treatment, and so soon as it could be borne, tr. ferri chloridi was given in five-drop doses every two hours. One month later the anæmia was less marked, but purplish, painful boils were scattered over the body and limbs in large numbers. Tr. ferri chloridi was again ordered in fifteen-drop doses three times a day. The patient was at this time involved in domestic quarrels, she was agitated, and she again had diarrhœa, with sugar averaging 115 grms. per 24 hours. The patient improved, the boils disappeared, and the sugar again fell to 68 grms. per 24 hours. August 13th, two months after the first effort, iodoform was again tried, this time in doses of .66 grm. or ten grains per day, five grains being taken in the morning, and five grains being taken at night. Eructation was not complained of by this patient, and her breath was not particularly offensive. She bore this dose very well, and the sugar diminished through two consecutive weeks, reaching an average of 30 grms. per 24 hours. The average then commenced to become higher, though true toxic symptoms were not developed until the fourth week, when diarrhœa occurred. During these four weeks the patient gained weight. This patient was subject to spontaneous diarrhœa. The iron was stopped, and the iodoform was continued to test more fully its effect. It was now given in doses ten grains night and morning, or 1.3 grms. per day, and it was continued for two more weeks. The quantity of sugar during the first week descended to an average of 34 grms. per 24 hours. During the second week it began ascending. Diarrhœa and trembling also occurred. The iodoform was stopped for two weeks, and the line fell during the whole period, reaching an average of 28 grms. per 24 hours. The treatment was recommenced in the 1.30 doses, and the sugar, with some oscillation, rose, reaching an average of 87 grms. per 24 hours, the highest point of the second period of medication. The quantity of urea also rose during this time from an average of 25 grms. to an average of 40 grms. per 24 hours. This period covered four weeks, and during the whole time the patient was suffering from nervous and intestinal disorders. The kilogramme of weight which had been gained during the previous month was lost.

If we exclude from our consideration all oscillations which do not persist through at least two weeks, the chart presents only three well-marked lines of descent in the sugar record of this patient, the line 1–2 representing a descent from 140 grms. to 65 grms. per twenty-four hours, obtained during iodoform medication; the line 3–4 representing a descent from 65 grms. to an average of 30 grms. per twenty-four hours obtained during iodoform medication; and the line 5–6 representing a descent from an average of 50 grms. per twenty-four hours, to an average of 28 grms. per twenty-four hours, obtained during an intermission to its use. These lines are further distinguished by their occurrence during periods which are not marked with the toxic symptoms of the drug. An elevation of the sugar ratio during toxæmia, is shown in the line which intervenes between 3–4 and 5–6, also in the remainder of the series. We may then say that in case 1 diminution of sugar was associated with the presence of the drug, and with the absence of the drug following its toxæmia, also that increase of sugar was associated with the toxæmia of the drug.

Case 2 commenced taking iodoform upon the fifth week, which is represented in the chart. For the month previous the quantity of sugar had been almost stationary at 65 grms. per twenty-four hours. During the week when the treatment was commenced, the urea averaged 50 grms. per twenty-four hours. The difference in the urea averages in these two cases results principally from the difference in their diet. As quoted by Voit,[1] the urea-averages for a man are: 15.41 grms. per day on non-albuminous diet; 22.48 grms. per day on vegetable diet; 32.50 grms. per day on mixed diet, and 53.20 grms. per day on strictly animal food. The diet of case 1 approached very nearly to the non-nitrogenous diet of this experiment, while the diet of case 2 approached the mixed diet of this experiment. Taking the question of diet into consideration, both cases present in about equal proportions the exaggerated urea excretion of the disease. Iodoform was given to case 2 at this time during five weeks. During four weeks, in doses of five grains, night and morning, or .66 grms. per day. During the first two weeks the quantity of sugar presented a primary increase, which is suggestive of the increase which preceded the descent 1–2 in the first series of case 1. During the third and fourth weeks of treatment, diminution of sugar to the average quantity of 45 grms. occurred. During the fifth week the dose of iodoform was carried up to two grms. per day. The trembling, which was already present during the fourth week, increased, and during the fifth and following weeks, the quantity of sugar steadily rose, reaching an average of 88 grms. per twenty-four hours. In the third week of the intermission toxic symptoms disappeared, and the sugar commenced to descend. The patient felt well, and iodoform was re-commenced in doses of ten grains night and morning, 1.30 grms. per day. This dose was well borne. For five weeks the sugar steadily diminished, and there was an increase of three kilogrms. in weight. During the fifth week trembling appeared. The dose of iodoform was reduced to .33 grm., or five grains per day, but the trembling persisted and sleepiness occurred, while both the sugar and the urea increased. The treatment was then suspended for three weeks. The treatment was resumed January 7, 1884, with a dose of ten grains night and morning, or 1.30 grms. per day. The response, this time, was immediate and rapid, but, the patient complaining of eructation, the quantity was reduced to .66 grm., taken in one dose, at night. In spite of the reduction, trembling occurred during this week. This "trembling" was choreiform in general appearance, and the twitching of single muscles could sometimes be observed. During this first week of the diminished dose, the sugar, and the urea also, was increased. At the commencement of the second week the patient had an attack, which was described in the following words: During the afternoon she was said to have commenced to feel "heavy." She was ironing, and, while at work, her arm became "powerless." About seven o'clock, she "fell upon the floor"; she "was not faint," but "her legs appeared to give out." This condition lasted about five minutes; ten minutes after she was able to walk. She went to bed as usual and slept, but was awakened early by a desire for stool, and between seven and ten o'clock she had about seven watery evacuations smelling strongly of the drug. She continued the ten-grain doses for the remainder of the week, felt very well, and the sugar and the urea began to diminish—as shown upon the chart. At the end of this period body-weight was found to be less, though intervening oscillations may have occurred. Many of her teeth were now found to be loosened, though she had not previously complained of salivation at any time. It is notable, that the albuminuria of this patient improved, and the question arises, whether the symptom was not produced by a cystitis which existed in the case.

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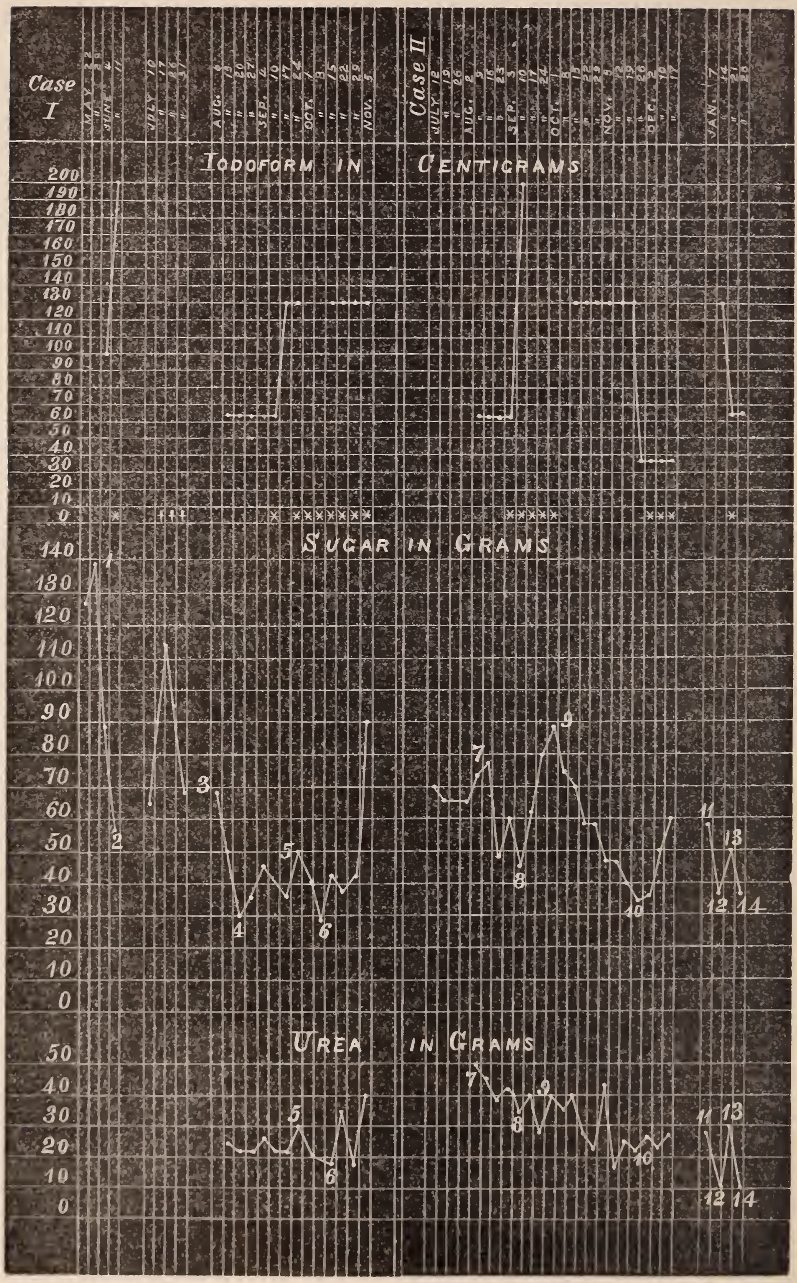
[1] Voit: *Hermann's Hdb. der Physiologie*, Bd. vi., Th. 1, S. 105.

During the first series case 2 presented two well-marked descents, the line included between 7 and 8 and the line included between 9 and 10. Both were associated with the use of the drug. The first lasted only two weeks, in this respect resembling the descents of case 1, while the second covered a period of eight weeks. Each of these lines of descent is followed by ascents which were, as in case 1, associated with the toxæmia of the drug. The second series of case 2 is unique in the prompt response to the medication and in the rapid and serious character of its toxæmia. It is also notable that toxic symptoms of so grave a character



* Toxic symptoms.

† Digestive and emotional disorders apparently unconnected with the medication.

should have been relieved by a diarrhœa, without the discontinuance of the drug. Case 2 took the tincture of iron during the whole course of her treatment. Anæmia did not appear. The vulvar eczema improved. General itching was not complained of in either case. In both cases the tendency to maintain the level of reduction may be noted. Two months intervened between the first and second periods of medication in case 1, yet, notwithstanding an intervening illness, the sugar was found at the commencement of the second series nearly at the level to which it had been reduced. Also between the second and third series in case 2 three weeks intervened; and it is significant that after this intermission the quantity of sugar was found not at the level which it had had before medication, but at very nearly the level at which it had been left.

Of both cases, then, it may be said that diminution of sugar and urea with an increase of weight occurred during the early part of the exhibition of the drug, that increase of sugar and of urea with diminution of the weight occurred in both cases during its toxæmia, that diminution of sugar and urea occurred spontaneously in one case after the toxæmia upon the discontinuance of the drug, also that the levels of reduction tended to be returned to, and to be maintained. In concluding the history of these cases, it may be said that they have migrated from our dispensary, not, however, on account of alarm or of dissatisfaction with their treatment. But case 1, on account of supposed personal neglect; and case 2, on account of the absorbing character of her household affairs. Both are known to be in their usual health.

The treatment of diabetes by iodoform was first proposed by Moleschott,[1] of Rome. Moleschott reported results upon five cases, as follows: case 1, a man sixty-three years, quant. of urine 2½ litres, spec. gravity 1017, whole quantity of sugar 19.4 grms. Case 2, a man forty yrs., specific gravity 1017, quantities not known. Case 3, specific gravity 1033, whole quantity of sugar 14.4 grms. Case 4, a man forty-six yrs., spec. gravity 1026, whole quantity of sugar 28 grms. Case 5, a man sixty-two yrs, maximum quantity of sugar 9.2 grms. Moleschott's doses varied between .10 and .30 grm. per day. The drug was combined with cumarin in order to disguise its odor, and was given in pill form. The sugar totally disappeared in case 1 with twelve days, treatment. Of the second case it is said that the sugar was found absent at the end of six months. In the other cases the sugar did not entirely disappear. In the third case the sugar had diminished from 14.4 to 1.6 grms. in three months. In the fourth case it had diminished from 28 grms. to 1.6 grms. in four months. In the fifth case the diminution was from 9.2 grms. to 6.1 grms.

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[1] Moleschott: *Wiener Med. Wochschr.* Nov. 17, 18, and 19, 1882.

Moleschott's formula is as follows :

℞	Iodoformi,	1.
	Ext. Lactuc. Sat.,	1.
	Cumarinæ,	0.1

Ft. in 20 pills. Cover with mucilage.

Dr. Moleschott had no unpleasant nor hurtful results. In considering the value of his conclusions it is to be regretted that the lactucarium was not omitted from his dose.

During what is called the iodoform debate before the Gesellschaft der Aertzte in Vienna, Prof. Drasch[1] presented a report of three cases treated according to Moleschott's method. The first patient was a woman of fifty-eight years. Her daily quantity of urine was 4 litres, specific gravity 1035, and her quantity of sugar 8 per cent. Her diet was restricted, and iodoform .1–.3 grms. per day was given, with cumarin, in pill form. The thirst became less during the first week. The urine during that period sank to 2 litres per day, and the sugar to 4 per cent. During the third and fourth weeks the whole quantity of urine was 2–4 litres, and the sugar was 2–4 per cent. After thirty days the sugar had not yet entirely disappeared. The patient had phthisis, and a hæmoptysis caused interruption in the iodoform treatment. The patient eleven days afterward died. At the autopsy an intense iodoform odor was obtained upon opening the skull. This odor was present in the lungs and in the abdominal viscera, but to a less degree. During the treatment the patient suffered with abdominal pain, and a more or less profuse diarrhœa, with a burning sensation of the fundament. She had eructations of the medicine, which were disagreeable, and which injured her appetite. She had in addition a torturing general pruritus, which was also ascribed to the drug. During the four weeks of treatment she lost 3 kilogrms. in weight. The second case had at the beginning 2 litres of urine, and sugar 1.96 per-cent. Iodoform was given in doses of .1–.3 grms. per day during five weeks. The thirst was diminished upon the second day. At the end of the five weeks the quantity of urine was 1½ litres per day, and the sugar amounted only to .43 per cent. The sugar did not entirely disappear. This patient also suffered with itching of the skin. The third case had 10 litres of urine per day, with sugar 6.8 per cent. Upon the first day of iodoform the thirst was diminished. After four days, however, the patient returned with so intense a pruritus that he scratched himself night and day until the blood came. The iodoform was stopped. Doctor Drasch concludes that iodoform does affect the thirst and the quantity of urine with the per cent. of sugar in diabetes mellitus, especially during the first days of its use. Also that the effect upon the sugar is more constant than that upon the quantity of the urine. He adds, however, that the itching, the diminished appetite, and the diarrhœa, sufficiently contra-indicate its application in the greater proportion of cases of the disease.

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[1] Drasch : *Wien. Med. Presse*, 1882, xxiii, 1487.

Bozzoli[1] says that De Renzi has experimented in Naples with good effect from iodoform in diabetes. Bozzoli himself reports two cases. In one case complete suspension of the glycosuria was produced, and in the other, a more grave case, a considerable diminution was obtained. He gives no details in regard to the cases. His dose was 1–2 grms. iodoform per day. He notes anæmia from its use.

Still more recently Sylvester[2] of Parma reports an unsuccessful case. The patient was a man of sixty-three years. His illness was of three years' duration. Quantity of urine, 3 litres, and quantity of sugar, 132.80 grms. This patient was placed on a meat diet April 19th. April 27th, the total quantity of sugar was 124.88 grms. Continuing the animal diet, .30 grm. iodoform was given per day in six pills. May 1st, the total quantity of sugar was 103.10. The treatment was continued. May 7th, the patient refused the meat diet, but iodoform was continued. May 12th, the sugar total was 178.86 grms., and the patient continuing to grow worse the medication was suspended. Sylvester concluded that the transitory diminution was entirely due to the diet. He does not think that the iodoform had any or but very little influence upon his patient.

Doctor Drasch has given salicylic acid for the sake of comparison with the iodoform results. He gave it in doses of 3.–15. grms. per day. He found that it did not affect the sugar so quickly as iodoform, while its incidental effects were quite as disagreeable. Moleschott also compares iodoform with salicylic acid. He considers it quicker and more certain.

In our own record the descents 1–2 and 3–4, also 7–8,

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[1] Bozzoli: *Gaz. degli Ospitali Milan*, Feb. 4, 1883.

[2] Sylvester: *La France médicale*, Oct. 23, 1883.

9–10, and 11–12, were produced during the exhibition of iodoform. On account of previous elevation in case 1 and on account of previous constancy in case 2, these descents are supposed to be due to the specific action of the drug. Compared with Moleschott's, our own cases were of a grave character, approaching to those of Prof. Drasch in degree. Prof. Drasch notes for his cases diminution of the thirst, of the quantity, and of the sugar, without the complete disappearance of the diabetic symptoms. He also notes that his results were most marked during the early part of the treatment. In our own cases the descents 1–2, 3–4, 7–8, and 11–12 are included within the first two weeks of the medication. While during the production of the descents 3–4 and 9–10 body weight increased, during the production of the descent 7–8 the body weight was diminished. Prof. Drasch notes for his case of diminished weight great digestive disturbance. The coincident diminution of urea during the period covered by the descent 7–8 in our own case shows that the loss of body weight was not due to excessive waste, and we also have ascribed it to the lack of appetite which was observed. When the weight increased during the production of the descents 3–4 and 9–10, the medication was well borne, the appetite was good, and the patients were in a comfortable state of health. The primary increase of sugar obtained at the point 1 and at the point 9, have not been previously observed.

To resume: in a moderately grave form of diabetes, both in Prof. Drasch's cases and in our own, iodoform has diminished the sugar; has diminished the urea, and has increased the weight in our own cases; and has diminished the weight with a diminution of sugar, in both sets of cases, where the appetite was impaired. The therapeutic action was not progressive but was limited to the early part of the treatment in both sets of cases. This close agreement between our own therapeutic results and those of this eminent clinician is presented as a basis for confidence not only in our lines of descent, but also, and still further as a basis for confidence in our lines of ascent also or the influence of the iodoform toxæmia, which has never yet been considered in this connection. Quite recently Gaetano Rummo[1] has, in the laboratory of Vulpian, very thoroughly investigated the physiological action of this drug. Previous authors are agreed in finding three sets of phenomena to follow its administration: first, gastro-intestinal troubles, anorexia, eructations, epigastric pain, vomiting, and frequently diarrhœa; second, cardio-vascular troubles, pulse 130–140, irregular, and sometimes feeble; and third, nervous troubles, psychic excitement, agitation, hallucinations, tendency to suicide, impulsive mania, and furious delirium followed by depression characterized by melancholia, with a tendency to somnolence, loss of memory, and a comatose state. Some authors find an elevation of temperature, others not. Rummo gave to two diabetics 2 grm. of iodoform per day for twelve days and found slowing of the pulse instead of augmentation, while the wave of blood elevated the wall of the artery in an energetic way. Patients complained of a persistent inclination to sleep, and afterward of insomnia, of a diminution of hunger, and of headache. Upon frogs Rummo observed marked slowing one to two hours after the injection of .02 grm. under the skin. At the end of three and a half hours the heart had stopped in systole. He says that only very large doses cause primary rapidity and irregularity; that, as a rule, the rhythm is regular and the contractions are strong. Rummo remarks that if we excise the heart poisoned by chloroform, its movements, when resumed, have their previous character. The iodoform heart, on the contrary, though previously extremely slow, will, after the removal, commence to beat in a normal way. He says also that after complete cessation contractions can be re-excited by the continuous current, that the slowing of iodoform is not abolished by atropine, and that if the vagi be previously cut iodoform produces no notable action upon the heart. Rummo concludes therefore that its action is effected through the vagi by stimulation of their centres in the brain.

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[1] Rummo: *Arch. de Physiologie*, Aug., Sept., and Oct., 1883.

In dogs primary augmentation of the arterial tension occurred. In frogs observation of the interdigital membrane showed at the commencement of absorption dilatation of the capillaries, but later a progressive contraction was present.[1]

This author finds also that iodoform in poisonous doses does affect the temperature, first increasing and then diminishing it. He gave 6 grms. to a dog. The temperature was previously 39.6° C.; three hours after it was 40.2°; at the end of twenty-four hours it was 37°; and upon the following day it was 33.5° in spite of the advent of tetanus. Rummo considers its influence to be upon the heat centres in the brain.

In reference to the nervous symptoms, Rummo experimented upon the frog and deducted first a general depression, and later a general exaltation of nerve function. He gave .03–.04 subcutaneously in the posterior extremity. Locally the muscle lost its electro-contractility. If the quantity injected were great, this loss was complete. If the injection were directly upon a nerve, the nerve lost its excitability. The general effect commenced one half to one hour after the injection. The animal became less agile; it could not remove from the dorsal decubitus. Electrization of the cord or of the peripheral end of the divided sciatic showed no diminution in excitability, and the muscular irritability was intact. The sensibility and the reflexes were good. The feebleness, however, was progressive, the member which was injected being the one which was the most rapidly impaired. Movements on galvanizing the leg became less pronounced than upon a healthy frog. The irritability of the muscle was less affected than was the conductivity of the nerve. The sensation was diminished, but less than the motility. The nerves were affected more than the muscles, and the motor more than the sensory nerves. This group of phenomena embrace the first period or that of depression. Later, contractures occurred in the leg which was injected, and these contractures progressively invaded the entire body. In the muscles, which were tonically contracted, fibrillar twitchings could be observed ; also isolated contractions, now here and now there, occurred. These isolated contractions would move now a limb, and again a side of the abdomen or breast. The reflexes at this time were augmented, but to a less degree than in the poisoning of strychnine. The rigidity continued after the section of the cervical cord. 3–4 grms. were given to dogs by the stomach, or by peritoneal injection. In dogs the first period was shown by profound sleep, a tendency to avoid light and sound, general feebleness, and anæsthesia—general, but not intense. The reflexes were somewhat diminished. The pupils were contracted but responsive to light. The faradic excitability of the cortical motor area was diminished, the animal having been previously trephined. The second period was ushered in by spasmodic paraplegia. Later the anterior members also became contracted. The animal walked upon the claws of these members, and at the same time crossed his paws one over or before the other. Trembling followed intentional movement. The reflexes were exaggerated, and spontaneous and provoked trepidation was present. Later the animal could not progress upon the four legs, but supported himself upon the anterior extremities while he dragged the posterior extremities behind him. The general sensibility was but little modified, and the intelligence was preserved. The pupils were somewhat dilated. Permanent erection with œdema of the prepuce was present in several instances. Still later, the tetanic contractions having become general, intense opisthotonos occurred. There was increased augmentation of the reflex excitability. All excitement, light, sound, electric, mechanic, and thermic augmented the spasms. The animal tended to rotate on its antero-posterior axis. Respiration became difficult, and the pupils were largely dilated. Transverse section of the cord did not prevent the convulsions, artificial respiration being maintained; and the enfeeblement was not prevented by ligature, which deprived the part of blood.

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[1] Balp has found diminished arterial tension with the invasion of anæmia (*Gaz. degli Ospitali*, Feb. 4, 1883.) Bozzoli connects the therapeutic action of the idoform with the anæmia and the diminished tension. Such a dependence does not appear probable, as in our own cases the diminution of the diabetic symptoms was still to be observed when the anæmia was prevented by the administration of iron.

For the effect upon the digestive tract Rummo experimented upon dogs. For gastric ingestion the iodoform was combined with mucilage or oil. If to the dog 1.–.50 grms. were given at a single dose, or .30–.40 grms. daily for several days, the animal ate as usual; he did not vomit, but he showed great thirst. If 2–3 grms. were given at one time, or 1 grm. for several days, the dog refused solid food, but still accepted liquids. Sometimes there were vomiting and liquid evacuations from this dose. The stools were yellow and sometimes contained mucus and blood. A dose of 4–5 grms. caused the animal to refuse aliment. It vomited. It would try to drink, but even the water would not be retained. The evacuations were frequent, and resembled those of diarrhœa, and the animal became rapidly emaciated. The gastro-intestinal trouble appeared only to a less degree when the drug was injected into the cavity of the peritoneum. Medium doses could produce this disturbance without any sign of local irritation of the mucous membrane of the canal. In other cases hyperæmia of the duodenum would be present, and the small intestine contained much mucus. In still larger doses the hyperæmia extended through the entire canal.

Nasal catarrh and eruption of the iodic type occurred.

The saliva, the bile, the urine, the pancreatic secretions, were increased. The secretions were obtained by means of canulæ, and upon the curarized dog, artificial respiration being maintained. In all instances it was noted that when the iodine reaction appeared in the submaxillary gland, the secretion was increased, the saliva and the bile becoming three times their usual quantity. If iodoform were given by the mouth, the iodine reaction appeared at the end of one hour to one and one half hours after its injection. If the medicine was dissolved in ether and injected under the skin, the iodine reaction appeared in ten or twenty minutes after its injection. The augmentation, as a rule, lasted only about forty minutes. During this time the quantity increased rapidly, and then diminished slowly until the normal was reached. The intestinal secretion was found to be increased, ocular demonstration being made upon the curarized dog. All the secretions were natural in character, except the bile, which was less viscid and less colored than is usual. Hypersecretion of mucus frequently occurred. The urine was at first increased. It sometimes contained albumen. When albumen appeared in the urine, the elimination of the iodoform was diminished.

In general terms it may be said that according to these observations iodoform does specifically affect the secretory, the nervous, and the circulatory functions. Iodoform is not locally irritating. When taken by the mouth, it does not induce inflammation in the gastro-intestinal canal. The secretions are increased, especially the secretion of the salivary, of the gastro-intestinal glands, and of the liver. The effect upon the nervous system is first upon the nerve centres, and later upon the cord and upon the nerve trunks, upon the motor rather than upon the sensory tract. There is first produced diminution of voluntary motility, and later a diminution of the excitability of nerve trunks. The first stage of this effect is depressive. Later exaggerated reflexes, permanent contractures, and tonic convulsions occur.

The physiological action of iodoform had been previously considered, notably by Bouchardat, by Righini, by Maitre, by Binz, and by Högyes. Rummo's deductions do not materially conflict with these authors,[1] while his results are fuller and better defined. In our own cases the diarrhœa, the trembling, the agitation, the paresis, and the loosened teeth all find their counterpart in Rummo's dogs. The diarrhœa is explained as a hypersecretion similar to that which is obtained from the purgative salts.[2] The pulse in our cases was observed only for quality, which always remained good. Concerning the nervous symptoms in our cases, the twitchings in case 2 and the restlessness and agitation in case 1 could not be called signs of depression. The stupor and paresis appeared only once. If we accept the fact of the precedence of depression over excitement, we shall be obliged to conclude either that the primary stage may fail to appear or that their exists a still further preliminary stage. Reference to our records will show that upon the invasion of any one of these symptoms the diminution of sugar was lost, and that if the medication were persisted in an actual increase of its excretion did occur. That this increase was not incidental is shown by the fact that upon the omission of the drug diminution again would occur. Such a descent after toxæmia is shown in the line 5–6 of the chart. The ascent of sugar sometimes preceded the toxic symptoms mentioned. This is shown in the ascent which follows 3–4, and in that which follows 11–12. Based upon the above, we have ventured upon the proposition that in diabetes an increased excretion of sugar accompanies the iodoform toxæmia, and that, therefore, toxic doses of iodoform are unnecessary in diabetes, and, further, that they are prejudicial to its therapeutic action. The existence of this principle can be traced not only through our own experiment, but also through all the other therapeutic efforts which have been reported. Moleschott had no toxic effects. Drasch mentions diarrhœa and abdominal pain, but he also mentions that the therapeutic action of the iodoform was limited to the first few days of its use. Of Sylvester's case it is said that the patient refused food. Iodoform has been given in phthisis pulmonalis first by Semola[1] and later by Möller.[2] Semola gave it in divided doses of .05–.50 grms. per day. He says that the hectic was diminished. He does not speak of unpleasant effects. Möller gave it to eleven patients in doses of .10 grm. per day. He notices improvement in weight, which was not progressive, and which covered only the first few days.

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[1] Nothnagel and Rossbach: "Hdb. der Arzneimittellehre," 1880, S. 343.

[2] Dr. Matthew Hay: *Jour. Anat. and Phys.*, July, 1883.

Compared with the quantities given by Moleschott, by Drasch, by Semola, and by Möller, our own doses were large. We adopted the doses of Bozzoli, whose paper we first read. The continuance of the medication is a factor of equal consequence with the dose. The single toxic dose of iodoform for a man is very large: 50 to 80 grms. according to Högyes.[3] Baruch[4] has used 6 to 10 grms. at one time in the puerperal uterine cavity without injury, and Rummo has found absorption from such a denuded surface very good. Prof. Dittel,[1] in the Vienna debate, said that some German physicians used 400 to 500 grms. at one time in external applications. Toxic symptoms have usually appeared after a prolonged use. One of Oberländer's[2] cases had taken it for eighty days. The slowness of the elimination of the drug may be noted. Capranaca, who made determinations for Moleschott, found in the urine, daily, only iodides sufficient to account for a small proportion of the iodoform taken. Three days after the cessation of the medicine, iodides still existed in the urine. In one of Prof. Drasch's cases, iodine was found in the urine seventeen days after the cessation of the dose. Accumulation of the drug is to be seen in our own cases, possibly, in the response to the iodoform medication, which occurred with increasing promptness during each successive period of treatment. Case 1 commenced her first medication upon May 31st, and the diminution of sugar occurred toward the end of the second week, Her second period of medication commenced Aug. 13th, and a marked response was found upon Aug. 20th, or at the beginning of the second week. Case 2 responded to the first medication only toward the end of two weeks; to the second, within one week; and to the third, within three days. This fact, together with the fact, that in case 2 at least the toxic symptoms became with each series progressively more grave, will suggest either a surplus of iodoform in the system, or the occurrence of a structural change. The persistency of the results of the iodoform also may be remembered in this connection.

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[1] Semola: *Allg. Wiener med. Zeitung*, July 25, 1882.

[2] Möller: *Wien. med. Presse*, 1883, S. 1675.

[3] Nothnagel and Rossbach: "Hdb. der Arzneimittellehre," 1880, p. 343.

[4] Baruch: *N. Y. Med. Jour.*, Jan. 5, 1884.

Binz[3] considered the effect of iodoform to be due to the iodine which constitutes $\frac{9}{10}$ of its weight. Rummo, on the contrary, considers it to be due to the drug in its own form. He advances, as a proof, the fact that large doses may be absorbed by the stomach and from the peritoneum without hyperæmia or any other sign of local disturbance; that iodine is not narcotic; also that iodoform is eliminated in its own character from the lungs. Further, the odor of iodoform in the various organs of the cadaver might also be mentioned. In the urine, and in the other secretions, iodoform is eliminated as an iodide. It cannot, however, be stored up as an iodide, because the elimination of the iodides is rapid.

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[1] Dittel: *Wien. med. Presse*, 1883, xxiii., S. 1487.

[2] Oberländer: *Centralblatt f. d. med. Wiss.*, 1879, S. 336.

[3] Binz: *Arch. f. exp. Path. u. Pharm.*, Bd. viii., 1877, S. 309.

The accumulation and the persistency of this drug must receive consideration—even in the exhibition of the small dose. It appears to us that intermissions should be provided for, just as is done in the administration of other cumulative medicines. According to our experience, iodoform can be happily administered in doses of .66–1.00 grms. per day only for about two weeks. The smaller dose is not, as has been shown, administered for a much longer time with benefit. Our experience has shown also, that the level of reduction will persist through an intermission of from two to four weeks, when it will be again diminished under medication in the same ratio as before. The full quantity of iodoform can be given in one dose at night, by which method eructations and anorexia are less apt to occur. It can be given in capsules or in pill form. Rummo has found that the stomach bears it better when dissolved in oil. Such a solution might also be put up in capsules. We have not had, in our cases, any gastric disturbance which depended upon the single dose; great care, however, was taken by dipping the capsules in paraffine to completely close them and to confine the odor.

The pathology of disease frequently has been elucidated by the character of its therapeutic agents. Rummo's autopsies showed as follows: When 2 grms. were taken by the dog only hyperæmia of the duodenum was present. A dose of 5–6 grms. produced hyperæmia throughout the intestines in disseminated patches and in longitudinal lines. The stomach contained a yellow fluid and the intestines diarrhœal matter. The liver was yellowed and shrunken. The spleen presented central ecchymoses and the kidneys were congested. Congestion was present throughout the cerebral pia and throughout the gray substance of the brain, of the medulla, and of the cord. The medulla was softened and of a deep rose color. With very large doses the white substance also was congested, and little ecchymotic points could be found in the gray matter. On the trephined animal the hyperæmia appeared when the animal abandoned itself to sleep and it was most marked during the convulsions. For microscopic examination the animal was killed one, two, or three days after the ingestion of the drug. The liver was found to be the first organ affected. Rummo says that its cells first lose their regular polyhedral shape and become dilated and vesicular. They contain a granular semi-liquid substance, and about the interlobular branches of the portal veins are little granules which color with osmic acid. In a degree more advanced the hepatic cells also contain these granules, but the peripheral cells of the lobule contain many more than the cells of its centre. No sign of inflammation is present. The portal vessels were often dilated and contained granules of fat. The biliary canals were not altered, except that their epithelium often showed fatty change. The epithelium of the stomach and of the intestines was in the same condition, also that of the gastro-intestinal glands. The heart and vessels did not escape. The inter-muscular, the inter-fibrillar, and even the intra-fibrillar substance was granular. Neither the parenchyma nor the sarcolemma showed, however, signs of inflammation. The kidneys were altered. If the poisoning were slow only fatty degeneration of the cells of the tubules and of the endothelial cells of the vessels was found. If the dose were very large, however, in the glomeruli the capillaries were dilated and an exudation more or less extensive occurred. In some of the glomeruli it was so abundant that the blood-vessels were compressed. In the perivascular fluid he observed yellow granules, which were probably due to the destruction of corpuscles; also lymphoid and red cells, which had probably emigrated from the vessels into the space. The medullary region was congested with blood and presented extravasations. The tubules showed degeneration of the epithelium and casts. In the nervous system the most marked changes were to be found in the dorso-lumbar region of the cord. The vessels were gorged with blood, and transudation and extravasation through rupture were found. The anterior horns contained many newly-formed cells, while the multipolar cells were diminished in volume and had lost their processes, their nuclei were obscure, and they were transformed into little piriform and ovoid bodies. The axis-cylinders and the sheaths were not materially altered. The same lesions were found in the psycho-motor region of the brain. According to these post-mortem observations iodoform would appear to attack particularly the central nervous system, the liver, and the ultimate tissue cells. It thus equally supports the two prominent theories of the disease. The trophic influence of iodoform suggests the theory of Pettenkofer and Voit,[1] who find the origin of diabetes in instability of the protoplasm with inability to appropriate oxygen and to effect complete oxidative change. Pettenkofer and Voit[2] consider fatty degeneration also to be produced by cleavage in the albuminous molecule without oxidation. Iodoform toxæmia produces fatty degeneration. Logically, therefore, it should increase the diabetic symptoms. Such an increase we have found to be one of its earliest signs. If for the therapeutic action of iodoform we suppose a primary stimulation of cell-power Pettenkofer's theory is sustained.

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[1] Pettenkofer and Voit: *Ztshr. f. d. Biologie*, 1867, S. 380.

[2] Voit: "Hdb. der Phys.," Bd. vi, Th. 1, S. 235.

While Rummo uses the phrase fatty degeneration in describing the changes of the iodoform toxæmia, he inclines to believe that the fat found in the cells is infiltrated from without. He also refers to the fact that Hoppe-Seyler, Ludwig, and others do not accept the theory of Pettenkofer, with the recent work of Lebedeff,[1] who has found distinctive fats stored up in the body unchanged. Further, Schiff's theory, newly advocated by Pavy,[2] finding the origin of diabetes in an abnormal activity of the liver, determined through nerve influence by a loss of vascular tone, this theory also is supported by the action of iodoform, which is said to attack the liver before it attacks any other organ, which contracts arterioles in the therapeutic dose, and whose influence upon the vagi must suggest an invasion of the diabetic centre in the brain.

To recapitulate:

1. Iodoform in doses of 1 to 2 grms. per day has diminished the sugar and the urea, and has increased the weight in two moderately severe cases of diabetes mellitus.

2. The therapeutic influence of the drug was limited to the first few weeks of its use.

3. Increase of sugar and of urea with diminution of weight accompanied the iodoform toxæmia in both cases.

4. The toxic symptoms here referred to were anæmia,

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[1] Lebedeff: *Arch. f. die gesamt. Phys. d. Mensch. u. Thieren*, 1883, xxxi., S. 11.

[2] Pavy: *Brit. Med. Jour.*, May 5, 1883, p. 8.

salivation, diarrhœa, agitation, trembling, choreiform movements, sleepiness, and paresis.

To conclude: It is proposed that in the iodoform treatment of diabetes, systematic interruption to the medication should be made. With a dose of 1 grm. per twenty-four hours, the interruption may be made at the end of from one to two weeks; the intermissions may consist of from one to two weeks, and the entire quantity may be given in a single dose securely sealed and taken at night.

A PLEA FOR THE SUBSTITUTION OF THE SODIUM IODIDE FOR THE POTASSIUM IODIDE IN THERAPEUTICS.

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AMIDST the prevailing scepticism in therapeutics, few remedies have better maintained the right to be considered curative agents than iodide of potassium in the treatment of tertiary syphilis. So firmly is our faith in the powers of this drug established, that failure is generally taken to imply either a neglect of the element of quantity, which is as important as that of quality in the use of drugs, or to throw a serious doubt upon the correctness of the diagnosis.

All the more therefore is it to be regretted that this drug produces, in addition to its curative effects, others, which are not only non-curative, but positively disadvantageous and injurious. This is true of all drugs. We have no therapeutic agent whose effect exactly counterbalances that of the disease which it is meant to cure ; the very nature of the human organism, and the manner in which it is affected by disease, preclude even the possibility of such a finely limited action. On the contrary, even that portion of the effect of a drug which is curative remains so only within certain limits, beyond which it destroys where it was meant to cure. It is therefore the office of the therapeutist not only to find a remedy for a certain disease, but to use it in such a manner that the ill effects will be reduced to the minimum. It is with this in view that I am led to call attention to the advantages to be obtained from the use of the sodium iodide, instead of the corresponding potassium salt.

It is well known that the administration of large or even moderate doses of the potassium iodide is very frequently followed by the annoying and sometimes alarming manifestations of the condition known as "iodism." As a result the patient and sometimes the physician is discouraged from the use of a remedy which appears almost as bad as the disease it is meant to cure. It is generally recognized that the therapeutic utility of the potassium iodide upon the general system is due to the fact that it is an iodide, and not that it is a potassium salt, and the employment of the potassium iodide, to the exclusion of all other iodides, has led to the belief that all of the injurious as well as beneficial effects of this salt are common to all iodides; and hence that the condition known as iodism must be endured, in order that we may avail ourselves of the benefits of the iodides. That is to say, the iodides as a class have been saddled with much of the toxic action of potassium salts. We seem to forget that potassium salts, even when found amongst the iodides, are still potassium salts, and must exert a potassium action. We affirm a certain physiological action for the potassium compounds, and forget it merely because the potassium happens to have another active element combined with it, whose effects we desire, while those of the potassium are not required. How illogical it is to state that a certain dose of potassium chloride, for instance, produces certain effects, and to overlook these effects entirely in the action of even a larger dose of the potassium iodide, and yet these are the very errors of which we are guilty when we assume that all of the undesirable effects of the potassium iodide are unavoidably connected with the use of the iodides. It might be claimed, however, that many of the beneficial effects of the potassium iodide are to some extent due to the fact that a potassium salt is used, and hence that it would be inexpedient in many cases to substitute another iodide, such as the sodium iodide, for the potassium salt. To determine this point it will be necessary to enter upon a more especial study of the therapeutic effects of the potassium iodide; what is due to the potassium nature, and what to the iodine constituent of the drug.

The principal effects produced by the administration of the potassium iodide in doses sufficiently large to give us the well-known therapeutic results can conveniently, for our purposes, be divided into two classes:

I.—Effects that are therapeutically utilizable.

II.—Effects injurious to the system and therapeutically non-utilizable.

In the first class are included (and here we shall only give those effects which have been recognized by all therapeutists):

1st.—A specific curative action upon the various manifestations of tertiary syphilis, and, in combination with mercury, upon secondary syphilis.

2d.—A reducing or wasting, vaguely termed "alterative," action upon hypertrophied lymphatic and thyroid glands.

3d.—The power of hastening the excretion of metals in chronic metallic toxæmia.

4th.—A curative power over chronic and subacute articular rheumatism and gout.

Other effects, such as a supposed curative action in non-specific, chronic, and acute diseases of the nervous system, non-specific inflammations of serous membranes, etc., I have purposely omitted. This has been done, not because the influence of the iodide in these cases could not be accounted for on a physiological basis, for this is true even of some of the most important therapeutic applications of drugs, but because the utility of the drug in these affections does not even rest upon a firm empirical basis. In some of these conditions the iodide is used merely because no other drug is indicated, and only for the purpose of doing something.

To the second class belong all other well-observed physiological effects which occur to a greater or less extent during the therapeutic administration of the drug. These are, in the order of their toxic importance:

1st.—A weakening effect upon the heart's action, a lowering of the blood-pressure, and slowing of the pulse (these results are produced only by medium or large doses).

2d.—A catarrh of the mucous membranes of the gastro-intestinal canal, resulting in loss of appetite, dyspepsia, and mal-nutrition.

3d.—A temporary catarrh of the conjunctival, naso-pharyngeal, and respiratory mucous membranes.

4th.—A peculiar cutaneous eruption.

Before proceeding to review in order these various effects, we must remember that our primary object is to study them with a view of showing that the effects coming under our first category are for the most part not due to the potassium nature of the drug, but to its iodine component, and that, therefore, we might reasonably expect similar beneficial results from the use of the sodium iodide, and that the second series of effects are, to a great extent, due to the potassium element in the salt, and might be avoided by substituting the corresponding sodium salt.

First among the therapeutically utilizable effects of the iodide of potash is the one which is best known, and upon which the chief utility of the drug depends, namely, the specific effect which it exerts upon secondary and tertiary manifestations of syphilis.

In spite of the numerous theories which have been advanced to account for the marvellous power possessed by this drug in this disease, we are still driven to the subterfuge of hiding our ignorance, by terming it a specific action. But yet, both theoretically and empirically, it can be shown that other iodides possess this power to an extent equal to that of the potassium salt. There is, in the first place, no other potassium compound, as such, which has any curative power over syphilitic processes. Again, in studying the physiological effects of the iodides, the sodium iodide is constantly used by physiologists in their experiments upon animals in preference to the potassium iodide, whenever the drug is to be injected directly into the blood, and always produces effects like those which the potassium salt produces. Finally, such authorities as Nothnagel and Rossbach assert that they have used the sodium iodide almost exclusively for the past years, where the drug had to be given for a long time, with results equal to those obtained from the iodide of potassium. In this connection it will, perhaps, not be out of place to refer to a case of my own, which is one of several undoubted syphilitics, in which I used the sodium iodide with perfect success. The patient was a married woman who had aborted, or given birth, to dead children at full term upon four successive occasions. Her husband gave an undoubted history of a chancre two years before marriage; the wife came under my care for severe osteitic pains of the sternum and tibiæ; she also suffered from severe headaches, and an ulcer on the upper half of the left leg, together with indolent boils which were scattered over the whole body, and some which had ulcerated. All the symptoms presented by this patient were removed by the internal administration of sodium iodide, and the external application of iodoform to the ulcers. It appears, therefore, that from a physiological and chemical standpoint, we are warranted in expecting the same results from the use of the sodium as from the potassium iodide in syphilis, while empirically we think some reliance should be given to the results obtained by the authorities above quoted.

The so-called "alterative" action of the potassium iodide is acknowledged by all to be due to the iodine element of the salt. Indeed, in enlarged thyroid or lymphatic glands, we combine the internal administration of the iodides with the external application of iodine to the enlarged glands. Any assimilable iodide—such as the iodide of iron—will produce the same effect as the potassium salt in these cases, and here, again, therefore, we are not restricted to the use of the latter compound, but can use the sodium iodide.

The power possessed by the potassium iodide of hastening the excretion of the metals from the system, is a firmly established one, especially for lead and mercury. Indeed, some authorities have explained the action of the iodides in tertiary syphilis, on the supposition that it causes the excretion of the mercury, which has been previously taken and stored up in the system. Of course, this theory can only be maintained by such as believe that the mercury produces the tertiary manifestations. However ridiculous such a theory may be, I refer to it here, for the purpose of showing how positively the action of the iodides, in hastening the excretion of the metals, is established. It is supposed that the iodides, when taken into the system, convert the metals deposited in the different organs, into soluble combinations, and thus enable them to be excreted from the system more rapidly. There is, certainly, no reason to suppose that the sodium iodide can not do this as well as the potassium salt; indeed, many authorities believe that the potassium salt is converted into a sodium compound in the system, and hence it is at least as logical to administer the sodium as the potassium iodide for the purpose under discussion.

Finally, we come to consider the curative power which the iodides exert over chronic rheumatism and gout. This is accounted for by different authorities in two ways: first, on the ground that the potassium iodide in an "alterative," as are other iodides, and that it hastens tissue metamorphosis; secondly, that it acts nearly in a manner similar to the other alkalies in rheumatism, changing the pathological condition of the blood upon which the rheumatic process depends. This theory of its action will not account for the undoubted brilliant effects which follow the administration of the iodides in this disease and which do not follow the use of the alkalies; indeed, were we to acknowledge the validity of this theory, we should certainly prefer other alkalies to the potassium iodide. By the alterative action of potassium iodide is here meant not only the power which it has of causing the joint deposits in chronic rheumatism and gout to disappear, but also includes the supposed property of the iodide of changing the faulty nutrition and tissue metamorphosis upon which the rheumatic process depends; but here, again, the fact that potassium iodide is changed to the sodium salt in the blood, would necessarily imply that the administration of the sodium iodide would be just as efficacious in this disease as the potassium compound.

We have thus attempted to show that, therapeutically, the sodium iodide is as valuable for most purposes as the salt now used; but were this all, we should hardly be justified in substituting for a well-known remedy one which is still comparatively unknown. It will not be difficult, however, to give positive reasons for a preference in favor of sodium iodide.

With this view, we shall take up the second class of effects resulting from the therapeutic use of the iodides.

Here, again, it will be necessary to examine into each of these briefly, for the purpose of determining which of these are necessarily connected with the use of the iodides, and which are rather the results of the potassium action of the drug.

All physiologists and therapeutists agree that all potassium salts, whose diffusion power is sufficient to admit of their absorption, exert, in large and medium doses, a weakening effect upon the heart's action.

Mickwitz, after bringing animals (cats) under the influence of curare, and thus eliminating the element of shock from the experiment, injected 0.05 grm. of potassium nitrate into the jugular veins. He found, as a result, a diminution of the blood-pressure and slowing of the pulse,[1] .0.20 grm. even caused death by paralysis of the heart. Bunge,[2] comparing the effects produced upon warm-blooded animals (rabbits, dogs, etc.) by potassium chloride, found that it would take fifty grms. of a potassium salt, administered by the stomach, to produce paralysis of the heart in man. This quantity, however, would act as an emetic, so that fatal poisoning could scarcely be produced in this way ; but these facts will serve to show that, when the heart is already weakened from age or disease, large therapeutic doses of potassium iodide, such as alone are efficacious in some pathological conditions, would be dangerous from the effect exerted by the potassium upon the heart. On the other hand, no such injurious action upon the heart is exerted by the sodium salts (Bogolepoff). Bœhm[3] has shown that the sodium iodide exerts no deleterious effects upon the cardiac muscle or ganglia. In this respect, therefore, the sodium iodide is far preferable to the potassium salt.

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[1] Nothnagel and Rossbach : "Handb. der Arzneimittellehre," p. 15.

[2] *Zeitschr. f. Biologie*, 1873–4, vols. 9 and 10.

[3] *Archiv f. exp. Path. u. Pharm.*, v., p. 337 (1876).

Next in order we find the gastro-intestinal catarrh as one of the frequent injurious effects of even moderate doses of the drug. In some cases the presence of such a catarrh proves an effective bar against the employment of the potassium iodide, and the physician is forced to content himself with inferior remedies. An analytical study of the physiological action of the drug will show that the catarrh of the gastro-intestinal tract is due to the salt as a potassium compound, and not as an iodide. We find that when muscular tissue is cut out of the living body and placed in a solution of a potassium salt (chloride), two to three per cent. in strength, that such muscular fibres immediately lose their irritability. If this exposure to the potassium salt be not continued for too long a time, immersion in a sodium-chloride solution will restore the muscle to its irritability. And, furthermore, we find that when healthy muscular tissue, cut out of the body, is placed in a sodium-cloride solution, its irritability will be *preserved* for a much longer period even than when immersed in pure water (Kühne). The salt-frog so common in the physiological laboratory is a practical instance of the stimulating action of sodium-chloride solutions upon the living animal tissue.

Kühne has shown that while potassium-chloride salts have a paralyzing effect upon muscular and nerve tissue with which they are brought in contact, sodium solutions (chlorides) restore and preserve the irritability of nerves and muscles to which they are applied. Keeping these facts in view, we must also remember that in the gastro-intestinal canal the potassium-iodide solutions come in direct contact with the walls of the stomach and intestines, and, we have every reason to believe, with the muscular coats of these organs, although a slight amount has perhaps been already converted into sodium iodide before the drug has penetrated as deeply as the muscular layer. As a result, we have intestinal and gastric dyspepsia, which necessitates the discontinuance of the remedy, or if the disease imperatively requires it at all hazards, then the patient must suffer from mal-nutrition as a result of the dyspepsia. Thus the physician finds that while he is striving to cure one pathological condition, he is substituting another none the less serious.

Many practical therapeutists, among others Prof. Seguin, recognizing the importance and cause of this gastric catarrh, always order the potassium iodide to be taken in large quantities of Vichy water. I have followed this plan, and have found that the iodide was much better borne than when given in undiluted (often saturated) solution. This follows necessarily from the facts above stated. I should here like to refer to the gastric catarrh produced by the administration of the bromides. I am convinced that here also the chief cause is the direct effect of the potassium salt upon the gastric mucous membrane; the so-called "bromide breath," to which the catarrh gives rise, is not distinctive of the bromides, but is present when a subacute gastric catarrh has been produced by the administration of the iodides or from other causes.

It is highly probable, then, that the most annoying and injurious complications which follow the continued use of large doses of potassium iodide are due to the fact that a potassium and not a sodium salt is used.

The catarrh of the respiratory and naso-pharyngeal mucous membrane, when it occurs, is probably the result of the iodine component of the drug; this is only of minor importance as far as the general health of the patient is concerned, and is variously accounted for by different authorities. The only theory as to its etiology which we shall mention is the one advocated in the latest edition of Nothnagel and Rossbach. According to these authorities, the catarrh of the nasal and respiratory mucous membranes occurs only when either free iodine is used externally, together with the potassium iodide internally, or when the potassium iodide which is used is impure, from the presence of some free iodine; in either case, it is the direct irritation of the free iodine, either in process of excretion, or when inhaled as a vapor, which causes the catarrh. If this be true, these effects will be avoided by care being taken to administer a pure potassium or sodium iodide.

What has been said as to the cause of this catarrh will hold good for the eruption also; the latter is the external manifestation of an irritation of the integument, just as the former is of irritation of the other mucous membranes.

Theory then being in favor of the use of the sodium iodide in preference to the potassium salt, it only remains to determine the dosage, and to state what results have already been attained by this substitution.

Since the iodine is the important constituent of both compounds, the sodium iodide ought to be more effective than the potassium salt, for equal quantities of the former contain more iodine than the latter. The atomic weight of potassium being 39.1, that of sodium 23, and that of iodine 127, there will be 127 parts of iodine in 166 parts of KI, while there are 127 parts of iodine in 150 parts of Na I. This difference in strength, however, is to some extent counterbalanced by the fact that sodium iodide is of somewhat lower diffusion power than the potassium salt. We should therefore expect, as has been found in practice, that equal doses of the two salts would give equal iodine effects.

It is claimed, then, for sodium iodide that:

1. It can be used therapeutically for almost all, certainly the chief purposes, for which potassium iodide is used, and, I believe, with similar beneficial effects.

2. That sodium iodide is more assimilable than the iodide

of potash, both locally to the digestive organs and to the general system.

3. That, as a result, many of the local and general undesirable effects which are produced by the potassium iodide, do not follow the use of the sodium iodide.

It is to be hoped, therefore, that the sodium iodide will be used by those whose clinical advantages admit of an extensive trial of the drug, so that a more extended experience may confirm that which a limited experience would seem to claim for this drug.

TRAUMATIC ANEURISM OF THE VERTEBRAL ARTERY—CURED BY COMPRESSION.

BY ROBERT F. WEIR, M.D.,

SURGEON TO THE NEW YORK HOSPITAL.

On the 8th of December, 1883, a man named Robert Adams, aged twenty-eight, was brought into my ward at the New York Hospital, having received a short time previously a stab wound in the right side of the neck, from a knife held in the left hand of his opponent, who faced him when striking at him. The patient said he had bled very freely, but his clothes were not much stained with blood, nor was he weakened or exsanguinated. When first seen by the house surgeon, no further bleeding was taking place from the wound, which was situated about three quarters of an inch below the lobe of the right ear, and just anterior to the sterno-cleido-mastoid muscle, transverse in direction and about half an inch in length. There was below this point and extending several inches downward an ovoid soft non-pulsating tumor running under and posterior to the mastoid muscle. All exploration of the wound was avoided and an iodoform dressing applied and secured by a compress and firm bandage.

December 10th.—Tumor has almost entirely subsided. Wound nearly healed. Pt. yesterday afternoon suddenly experienced almost complete paralysis of sensation and motion of the left arm and hand. This was preceded by a " queer " confused not painful feeling in his head, momentary in duration. Consciousness not lost.

December 15th.—Tumor has disappeared. Wound entirely healed. Paralysis is slowly disappearing. At a point two inches below and one inch posterior to lobe of right ear palpation discovers a faint pulsation slightly expansive in character, and on auscultation a slight bruit is audible.

December 25th.—Since the last record all the signs of an aneurism have developed at the point indicated in last note. Pressure on the carotid just below the level of thyroid cartilage does not affect the pulsation in the tumor, but pressure over the tubercle of sixth cervical vertebra controls it at once ; nor does it react as long as the compression is continued. Ice-bags and pressure were ordered to be applied alternately every three hours over the tumor.

January 1st.—The signs of aneurism, including pulsation, thrill, tumor, and bruit, are still more pronounced. No appreciable effect has followed the treatment by ice and pressure. The area of pulsation is now nearly 2½ inches in diameter. The paralysis of the left arm is becoming less marked.

January 3d.—To-day digital pressure was resorted to at the lower anterior edge of the aneurism, where yesterday it was found that compression arrested all pulsation. This was continued for seven hours by the house staff, assisted by relays of students. Considerable force was necessary at first to control the circulation in the sac, which occasioned some pain and discomfort, and required morph. gr. ⅓ hypodermically to keep the patient quiet. No cerebral symptoms occurred at any time during pressure. After two hours there was great diminution in force of pulsation, and slight pressure controlled it without further discomfort to the patient. After three hours the pulsation could not be felt, and all signs of the aneurism save the resistance due to the tumor on palpation, had disappeared. Pressure was continued lightly until 7 P. M. (in all 7 hours), at which time a firm graduated compress was applied. No cerebral symptoms were noticed during the treatment.

No return of symptoms. A solid tumor can be appreciated at site of aneurism. Paralysis of arm has almost completely disappeared. General condition excellent.

January 12th. Since last note pt. has been up and about ; no return of sign of aneurism. Only a slight trace of the swelling can now be felt. The patient was to-day discharged from the hospital as cured.

The above case so happily terminated by a bloodless procedure is interesting from the general rarity of the injury, and also from the fact of its being the only one successfully treated by compression. According to Fischer,[1] in 32 cases of injuries of the vertetral, there were only 9 cases of aneurism, and all of these resulted from stab wounds. The most satisfactory résumé of the few cases of vertebral aneurism on record has been given by Holmes, in his justly celebrated lectures on the "Surgical Treatment of Aneurism in its Various Forms,"[1] which is herewith appended with the addition of Cattolica's second case.

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[1] Krankheiten des Halse : *Deutsche Chirurgie*, Lief. 34, 1880.

The significant fact in the above collection is the error so frequently fallen into by the surgeon, in taking the injured vessel to be the carotid or one of its branches. In all these instances the carotid artery was tied without benefit, or to the detriment of the patient. The only case mentioned by Holmes of neurosis from a vertebral aneurism is that of Möbres, in which the external carotid was exposed for ligation, but on applying pressure to the denuded artery no effect was produced on the aneurism, and the operation was abandoned after the patient had refused to submit to the laying open of the tumor. Ultimately the aneurism ceased to beat under the application of cold, and disappeared. The cessation of pulsation was preceded by very severe pain.

Since the date of Mr. Holmes' paper I have been able to collect but one other case besides my own, that of Kocher,[2] which is included in Fischer's cases. This brings the number of recoveries up to three out of ten. Kocher's case was also from a stab made in a man, forty-two years old, between the fifth and sixth cervical vertebræ. Three weeks after the reception of the wound, during which time several hemorrhages occurred with the formation of a pulsating tumor, Kocher enlarged the wound and removed the laminated clots, etc. He was able to seize the bleeding point with a long forceps, but could not apply a ligature to it. He then introduced a little pledget of lint dipped in the perchloride of iron, and crowded it in the space

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[1] *Lancet*, July 26, 1873.

[2] *Arch. f. klin. Chirug.*, Bd. xii., 1871.

SURGEON.	AUTHOR.	COMPRESSION TRIED OR NO.	DEATH AT WHAT PERIOD. POST-MORTEM APPEARANCES.
1. 1829, Chiari.	*Filiatre Sebezio*, ann. 3, fasc. 2; Chassaignac, "Traité des opérations," p. 334.	No.	Death nine days after ligation of carotid from pyæmia. Aneurism situated on the vertebral artery between the first two vertebræ.
2. 1834, Romaglia.	*Ibid*, and *Froriep's Notizen*, vol. xli., p. 89.	Compression on the carotid seemed to diminish the pulsations.	The carotid artery was exposed, and a ligature put round it, but not tightened. Died from hemorrhage. The aneurism was seated on the first curve of the vertebral artery, which in this case lay between the second and third vertebræ, instead of the first and second.
3. 1836, Cattolica.	*Gaz. méd.*, 1836, p. 435.	No.	Wound of artery high upon the neck. Carotid tied. Death from subsequent rupture of the sac.
4. *Ibid.*	*Ibid.*	Yes, without avail.	Carotid exposed for ligation, but not tied, as pinching it was found not to stop pulsation in aneurism. Aneurism sloughed.
5. 1840, Kluyskens.	Chassaignac, *op. cit.*, p. 239.	Compression stopped the pulsation, but the ligature of the carotid did not.	Death four months afterward from hemorrhage, after the bursting of the aneurism. No post-mortem examination.
6. 1847, Northern Hospital, Liverpool.	*South's Chelius.*, ii., 250.	No.	Died fourteen days after operation of tying the carotid from rupture of aneurism, which lay between the transverse process of the fourth and fifth cervical vertebræ.
7. 1863 (?), at St. José Hospital, Lisbon.	*British Med. Journ.*, vol. i., 1863, p. 197.	No.	Death on twentieth day after ligature of carotid. Cause not given. Aneurism situated between second and third vertebræ.
8. 1865, Lücke.	*Langenb. Archiv.*, vol. viii., p. 78.	Compression on the carotid diminished but did not quite stop the pulsation.	As the symptoms persisted after the ligature of the carotid, the aneurism was injected with perchloride of iron. It burst and was laid open, and the bleeding vessel commanded by pressure. He died more from the consequences of the ligature of the carotid than any thing else. Vertebral wounded between atlas and occipital bone.

between the transverse processes of the vertebræ, and secured it by graduated compresses of carbolized gauze, fixing the head in an immovable position by a stiffened cravat. The patient progressed steadily to a cure.

In the two cases where the diagnosis was made, the simpler measures of digital compression and direct compression on the wounded vessel have sufficed to cure the aneurism. It is probably unnecessary to more than allude in condemnation to the proposed ligation of the vertebral either at the root of the neck, preferably from behind the sterno-mastoid, or at the space below the transverse process of the first cervical vertebra, where the tortuosity of the vessel to accommodate the free rotatory motions of the head allows it to be easily reached. Unfortunately an aneurism is most commonly located in this region, and overlaps or obscures the artery so as in all probability to render its ligation under such circumstances difficult. The plugging after exposing the injured artery has also been shown by Kuster,[1] in two cases of wound of this vessel, to be easily accomplished. For this the plug or tampon of iodoform is to be preferred.

The principal difficulty in the cases of hemorrhage or aneurism in this locality is to determine whether the carotid or its branches, or the vertebral artery is sundered. This point is to be arrived at by carefully considering the effect of pressure on the carotid not only above but also on a level with the transverse process of the sixth cervical vertebra, or rather over the anterior or so-called carotid tubercle. Compression above this point affects, as a rule, the carotid artery only ; over and below the transverse process compression will not only occlude the carotid but also the vertebral artery, which runs to its outer side. It must be remembered, however, that the vertebral instead of entering the foramina in the transverse process of the sixth cervical vertebra in its upward course sometimes remains exposed until it enters higher foramina. To meet such possibilities it has been suggested to isolate the carotid according to Rouge's plan, which is, with the sterno-mastoid relaxed to grasp the muscle with the finger and thumb and to pinch the carotid between them. It is necessary also to keep in mind in trying to effect vertebral compression that the carotid tubercle is, according to Chassaignac, always from two to three inches above the clavicle.

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[1] *Berlin klin. Wochenschr.*, Nov. 26, 1883. Allusion is made to a successful cure of aneurism by Warren, but I have not been able to obtain the details.

A traumatic aneurism whose pulsations are unaffected by compression of the carotid applied, as above detailed, will be found to concern the vertebral artery, and should be treated first by ice and compression by bandages, or by the fingers, failing which I should deem the opening the sac, with the finger introduced to close the arterial opening until the fibrine clots are cleared away by the enlargement of the wound, and the subsequent firm antiseptic plugging most likely to bring about a cure in the rare cases of this injury.

STATIC ELECTRICITY IN MYALGIA.

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THE re-introduction of static electricity as a therapeutic agent has led some to its indiscriminate use in all cases in which electricity is supposed to be indicated, and this has resulted as might be expected in very frequent disappointment. It is important, therefore, that data should be collected as to the classes of cases likely to be benefited by its use, and those in which it is inapplicable or of inferior value.

It is evident that when the entire person is charged with electricity, and the discharge is effected at a given locality on the surface, the process is widely different from simply making a portion of the body a part of a circuit. In the former case the electricity passes from all directions toward the point at which the discharge takes place. It thus acts upon a large extent of tissue, the intensity of the action increasing as the above-mentioned point is approached. In the latter case the action is limited to the path of least resistance between the two poles, and there is no similar concentration of effect from all directions at the point of emergence.

All this is independent of any different quality in the agent itself. But that such a difference exists is undeniable. The greater diffusibility of static electricity as compared with other forms is perhaps its most striking peculiarity. It is difficult to confine it to a conductor, however good, and even by the aid of the most perfect insulation. This peculiarity can scarcely be without effect in its therapeutic application. Instead of following exclusively the channels of best conduction, its tendency must be to pervade the tissues. We see how readily it passes through non-conducting substances, such as paper, for example, and there seems to be no good reason why it should not do the same in the case of animal tissues.

What might be assumed *a priori* as to difference in effect is confirmed by clinical experience, and there is no longer any doubt that while static electricity cannot be substituted for the other forms in all cases, there is a field in which it is more useful than galvanism or faradism. As illustrating its value in myalgia, the following cases are of interest:

Case 1.—Acute lumbago. P. D., aged 42, coachman, received a thorough wetting from a sudden shower, and sat for some time after on the box exposed to a cutting wind. This resulted in lumbago so severe that it was with difficulty he could get to my office. His body was bent forward, and he supported himself by placing his hands upon his knees. Static electricity was applied to the lumbar region with considerable energy, nearly cutting him in two, as he expressed it. The first application resulted in enabling him at once to straighten up and walk erect. A second application, on the following day, removed the remaining soreness, and he resumed his position on the box. There was no return of the lameness.

Case 2.—Chronic lumbago. J. B., 'longshoreman, presented himself October, 1883, at the clinic, at the Post-Graduate School, with a history of lumbar myalgia, which had completely disabled him for several months. His body was bent forward, and he walked slowly and with great care, the slightest jar causing great pain in the back. If he stooped down he was unable to straighten up again without the assistance of his hands placed upon his knees. He had been treated with liniments, plasters, etc., but with no benefit. On the following day I applied static electricity, using nearly the full power of the machine, and continuing the application for ten minutes. When he stepped down from the insulating platform he was able to pick up an object from the floor, and to rise up again without the assistance of his hands, and with very little pain. He left my office walking erect and with a confident step. Two days afterward he returned, stating that he felt like a new man. There was still, however, some lameness and stiffness in the back which were entirely removed by two more applications.

CASE 3.—Myalgia of the muscles about the hip. John B., aged six years, who had been for some time under my care for cardiac disease, was found one morning to be unable to walk, on account of extreme pain on attempting to flex the thigh.

He was brought to my house in a carriage, and carried up the steps and into the office. On examination, I found that there was no pain when the limb was at rest or when it was moved passively, but attempts at voluntary motion caused severe pain, which was located principally in front of the hip-joint, and seemed to be the result of tension of the muscles and fascia in that region. There was slight tenderness on pressure, but no heat or swelling. Pulse and temperature normal.

Patient was placed on the insulated platform, which was connected with the positive side of the machine. Sparks of slight intensity were then drawn from the affected part by means of a small electrode applied outside the clothing. The application was borne with very little complaint, and at the end of about ten minutes the patient stepped without assistance from the platform and walked with ease about the room and to the carriage. On the following day there was a sense of weakness in the limb, but no pain or soreness.

CASE 4.—Myalgia in the shoulder and arm.

Mrs. T., aged thirty-six, had two years ago an attack of acute rheumatism affecting the knees and hips, and confining her to the bed for ten days. Since then she has had several slight attacks, mostly in the upper extremities. Early in January, 1884, the flying pains from which she had suffered more or less for some time seemed to settle in the left shoulder and arm. There was no general fever and no local heat or swelling, but a slight local tenderness. Movement of the shoulder was decidedly painful. These symptoms were entirely removed by a single, rather energetic application of static electricity, and there has thus far been no return (March 8th).

Case 5.—Myalgia of the muscles of deglutition.

Miss F., aged fifteen, came to me to be treated for a "sore-throat." For ten days she had suffered considerable pain in swallowing and a constant more or less severe aching in the throat.

Inspection of the fauces showed the mucous membrane to be in a normal condition. Pressure on the base of the tongue with a spatula gave pain, as also pressure from without above the hyoid bone. There was no heat or swelling. The case was clearly one of myalgia located in the muscles about the base of the tongue.

Several folds of a towel were wrapped about the neck, and through them sparks of moderate intensity were drawn, the sitting continuing five or six minutes. Considerable relief was immediately obtained, and two more sittings on alternate days effected a permanent cure.

Case 6.—Acute myalgia of the intercostal and abdominal muscles. Dr. T. H. B. called at my office on the afternoon of February 29, 1884, suffering from acute pain in the left side, extending from the sixth rib to the pelvis. His body was bent toward the affected side, and he found it impossible to stand erect. He had been in that condition for about six hours, and the pain was rapidly growing worse. Static electricity applied for five or six minutes enabled him to straighten up and to leave the office in a comfortable condition, and to attend the theatre in the evening. The pain did not wholly cease until the following evening, but became less and less after the application.

Case 7.—Chronic myalgia of one arm and knee.

Mrs. S., wife of a physician, had suffered for several months from muscular soreness in the right arm and wrist, resulting from a strain received in a fall. There was also a spot on the inside of the leg below the knee-joint where muscular action produced extreme pain, rendering it impossible to fully flex the knee. For six months she had not been able to kneel.

Three applications of static electricity on alternate days entirely removed the trouble in the arm and wrist, and so far relieved the soreness in the knee that the patient, with care, can now kneel without pain. Probably a few more sittings will result in a perfect cure.

The above are selected from a considerable number of cases of myalgia in which I have found static electricity decidedly beneficial. Indeed in any case in which muscular action is painful, if inflammation can be excluded, I should confidently expect benefit from this agent. In purely neuralgic pain I have not been so successful in its use.

The fact that this form of electricity can be applied through the clothing is a very great recommendation on the score of convenience to the physician and of acceptability to the patient.

PRE-THYROIDAL CYST—LARYNGEAL ABSCESS AND FISTULA—CURE.

By RAMON DE LA SOTA, M.D.,

SEVILLA.

The subject of this history is J. F., a woman, twenty-six years old, single ; tall, well built ; of nervous, lymphatic constitution, without any pathological antecedents except the ordinary sufferings which are peculiar to childhood. There is no diathesis whatever in her family.

When, on December 9, 1881, I saw her for the first time, she had been suffering from her throat about two months, in consequence, she said, of a very severe excitement. This, I learned, was a quarrel that lasted over an hour, and in which she stood her ground with that impetuosity common to low-bred Andalusian women. Immediately after she took from a jar a drink of water which was very cold.

At once she experienced a hot impression in the larynx, feeling as if a foreign body was lodged in this organ. Afterward she commenced to feel some stinging pain ; a dry and frequent cough set in, and a persistent hoarseness. All these symptoms became gradually worse, at the same time that there appeared in the anterior part of the neck a lump which rapidly augmented. Breathing and deglutition had not been materially changed till the previous week ; but since, swallowing and breathing were getting more difficult every day. She often experienced an attack of suffocation of brief duration, and sometimes, on account of the severe pain in her throat, she could not swallow her food, but was forced to throw it out after having it a little while in the mouth. Except sleeping, all her other functions took place regularly ; her physical and mental sufferings prevented her from sleeping, and she was exceedingly frightened on account of her illness.

On examining the neck I found in the anterior part of the infra-hyoidal region a fluctuating tumor, of about the size of a hen's egg, not sensitive to lateral displacement, but which gave her great pain if pushed from front to back. The skin covering it had the usual color and consistency, and was not adherent to the tumor, as it glided with the greatest facility in every direction, and a fold of it could be raised everywhere.

But as the tumor moved up and down with the larynx during deglutition, and it was quite impossible to pull the two apart, there was no doubt but that it was adherent to this organ.

The patient's voice was hoarse and scarcely audible ; her cough dry and cracked ; her breathing somewhat difficult ; and when, following my directions, she swallowed saliva, her face revealed signs of acute pain.

The laryngoscopical examination showed an inflammatory state in the whole pharyngo-laryngeal mucous membrane ; but the tumefaction and the deep-red color of this membrane was most remarkable on the posterior surface of the epiglottis ; in that part, situated just under the anterior angle of the vocal cords, a swelling of the size of one half of a hazel-nut, and of a very deep-red color, could be noticed.

In the part of the neck corresponding to the lowest portion of the external tumor I introduced the needle of Pravaz' syringe, and with this instrument made aspiration, but failed to draw out any quantity of the liquid supposed to be there. Nevertheless, I persisted in believing the tumor to be a cyst of the pre-thyroideal serous bursa, as the point of the needle moved freely in every direction, instead of being held by a solid mass. Drawing out the needle, I laterally compressed the tumor, and then there came out, through the opening of the puncture, a stringy transparent liquid, very much like the white of an egg. I stopped before completely emptying the cyst, as the patient suffered a good deal, and asked me not to trouble her any more for the moment.

Three days afterward she came back to the Polyclinic with the same subjective and objective symptoms, and I repeated the compressions till the sides of the bursa were in mutual contact. On finishing, I heard a crack, and at the same moment the patient was attacked by a severe humid cough, accompanied by a slight whistling through the aperture in the neck, and she expectorated some purulent sputa, with some streaks of blood and of a very fetid odor. With the laryngoscope I observed that the subglottic tumefaction had considerably diminished. Through the cutaneous opening I introduced a probe, that I moved about in every direction; I touched the thyroid cartilage, which was altered and rough, and under its inferior border the probe entered the larynx, producing a severe paroxysm of cough. I then gave the probe to one of my assistants to hold, and by means of the mirror I perceived it just in the middle of the larynx, it having passed through the anterior wall precisely at the point where the infra-glottic tumefaction had been seen before.

On the 26th of December my patient made her appearance at the Polyclinic again.

The external tumor had not re-acquired its previous size; the internal one was entirely gone and in its stead a granulating surface was seen, in the middle of which there was the internal opening of the fistulous passage going from the larynx to the skin of the neck. Out of the external aperture there was continually dropping a viscous clear liquid. There was neither dyspnœa, dysphagia, nor spontaneous pain, but the latter was provoked by coughing. The cough was not hard, and was always followed by muco-purulent expectoration. Every effort forced the air through the fistula, and then a clear whistling sound was heard. The probe introduced through the fistula passed freely into the larynx, producing a violent cough, and was immediately found in the laryngoscopical image. The pharyngo-laryngeal inflammation was not so intense as heretofore.

I passed vertically through the tumor a filiform seton, availing myself of the puncture that I had made with the needle of Pravaz' syringe on the first day, which puncture now was the external opening of the fistulous passage; and with a small portion of salicylated cotton, soaked in a solution of chloride of zinc, I touched the diseased mucous membrane.

On the 3d of January all the subjective symptoms, with the exception of a slight pain produced by the touch, or by coughing, had disappeared. Deglutition and respiration were quite good. The voice was clear. The patient was joyful, had a fine complexion, and was getting stouter. The cavity of the cyst was greatly reduced; it was noticed that the probe could not move in every direction as before; the only route that it could now take being the passage of the seton and the laryngeal fistula. Through the cutaneous opening of the fistula there flowed a homogeneous pus of good character. There were only some granulations in the pharyngeal mucous membrane; the laryngeal was entirely healthy with the exception of a small portion under the angle of union of the vocal cords, corresponding to the internal fistulous opening, where a little tumor of the size of a small pea could be seen. The whistling sound produced by the passage of air through the fistula was still heard. The same treatment was continued.

On the 10th of January the cavity of the cyst had entirely disappeared; only with great difficulty could the probe penetrate into the larynx. I removed the seton.

During this month nothing was done but washing with carbolized water all the affected parts; but on seeing that there was no cicatrization except at the superior opening of the seton, I made at the end of January an iodined injection through the fistulous passage. The liquid entered the larynx, producing violent cough and troubling considerably the patient who, for some time, perceived a strong taste of iodine.

During the month of February the injection was twice repeated without any material benefit, and the patient's aversion to iodine became so great that on the second of March I used in its stead a strong solution of chloride of zinc; this injection was repeated once a week. Soon after, a great improvement was observed: in the month of April the internal opening disappeared; the air no longer escaped through it, and the probe could no longer be made to reach the larynx. Some exuberant granulations formed in the cutaneous wound, and, in order to destroy them, I applied to them nitrate of silver. Afterward there remained an ulcer which, a few days later, was again covered with granulations; I repeated the silver application several times, till the 26th of May, when cicatrization was complete.

One year after this I had the opportunity of seeing the patient, who was perfectly well. The only sign of her illness that had remained was a hollow scar adherent to the subcutaneous tissues.

Remarks.—The case which I have just described seems to me somewhat remarkable in its production as well as in its course and termination.

After an excited quarrel, in which the efforts of the voice were neither slight nor of short duration, a woman takes a drink of cold water and, immediately after, felt something in her throat. So far there is nothing remarkable; nor is it astonishing that a laryngitis more or less intense ensued.

But it is not what we generally observe, that the laryngitis was followed by a perichondritis, and this by a cyst of the pre-thyroidal bursa; the patient being free from any constitutional affection, as was proved by the preceding account and by the relative facility of cure. Some authors, Tobold among them, affirm that they have seen cases in which refrigeration alone has been the cause of perichondritis. I had previously always found this affection as symptomatic of some acute illness, such as typhus, small-pox, scarlet-fever, etc., or of some chronic disease such as syphilis, tuberculosis, scrofula, lepra, etc. And this is the only time that I have known it to be produced by any local and accidental cause.

The development of the cyst is yet more strange. This I consider due to the transmission of the inflammation from the perichondrium to the serous pre-thyroideal bursa, in which the phlogistic process determined such a preternatural secretion, as to rapidly fill up its cavity and distend its walls.

That the cyst was formed by an excessive functional action and not in consequence of an exudation of blood, was proved by the character of the liquid therein, so very similar to that which is found in ranula. The formation and existence of ranula Malgaigne attributes to hypersecretion of mucous glands and not to that of serous bursæ; but this idea is inapplicable to the pre-thyroidal space.

I presume that the abscess was imperfectly opened in the cavity of the cyst between the patient's first and second visit; when, by compression, I broke it in order to completely evacuate the liquid, the pus poured out into the larynx; and the severe attack of cough which expelled it from there, forced the air to pass whistling through the narrow fistulous passage. I do not consider it probable that the rupture simultaneously took place in the laryngeal and cyst cavities.

I have stated that not only the antecedents but the relative facility of the cure prove that the patient was entirely free from any constitutional affection. If her disease had in any way been the result of the general state of the patient, the radical cure of a morbid process so very complex could never have been attained through local treatment; at the very best, only a temporary closing of the fistula could have been obtained.

While I had this case under observation, there came to my private office a student of medicine, a brother of a distinguished professor of the University at Barcelona, who, having neglected the first periods of a constitutional syphilis, came to me with a gummy tumor in the infra-hyoidal region, which had abundantly suppurated, and finally formed a complete laryngeal fistula. By means of a vigorous general and local treatment, all the injuries in the neck and larynx entirely disappeared; the only visible signs of the fistula which afterward remained were a radiating hollow scar of a brown hue on the skin and a notable fold in the right half of the epiglottis, produced, no doubt, by the retraction of the cicatrix from the internal fistular opening. When this patient felt himself free from all his complaints, he gave up coming to my office, and thinking himself cured, he renounced all treatment. The summer of 1882 passed, and with it disappeared the supposed restoration. After a few months of continual suffering, this young man came to consult me again; his larynx was greatly injured, and the fistula open again. All my endeavors to save the life of this unfortunate person were useless; he died last April a prey to laryngo-pulmonary phthisis of a syphilitic nature.

I used the filiform seton for my patient's cyst, because in the treatment of ranula it has always given me a satisfactory result. And having obtained also a good success in the present instance, I recommend its use, as well as the injection of the fistulous passage with a solution of chloride of zinc.

I will not conclude without stating that, as there were no alarming symptoms, and as I thought that the purulent collection was not entirely formed, I did not introduce into the larynx, under the guidance of the mirror, the lancet with concealed blade, in order to cut open the intralaryngeal tumor, after having made the first laryngoscopical examination. But had dyspnœa indicated that the patient's life was in danger, no other consideration would have kept me from incising the laryngeal mucous membrane as far as I should have been able.

A CASE OF ACUTE ATROPHY OF LIVER—WITH MARKED DELIRIUM.

BY JOHN BOWEN, M.D.

THIS case presents no very unusual symptoms, except, probably in the violence of the delirium. Its chief interest is that it was mistaken for a case of acute mania, and thus sent to the insane asylum.

When patient was admitted to the asylum, her condition was such that she could give no history of her illness, and we were unable to get at her friends until after her death; thus, though we were satisfied that the diagnosis of acute mania, made by the two physicians certifying to her insanity, was incorrect, and that the patient had delirium of acute disease, we were unable to determine what the acute disease was.

The following history is given as it appears on the record of the asylum:

Sarah K., æt. twenty-three; single; born in Ireland, and has resided in New York for the past eighteen years.

Patient was admitted to the Kings County Insane Asylum in the evening of July 17, 1882.

The certificates of the physicians certifying to her insanity set forth that "she has attacks of acute mania. She raves incoherently on religious subjects, refuses to eat; attempts to escape from the house; has to be restrained by force, etc." On arriving at the asylum patient was calling loudly for the "Mother of God," and, as she resisted mechanically every effort to assist or direct her, had to be carried forcibly to the ward. On reaching the ward, it was noticed that the conjunctivæ showed jaundice. Patient walked the floor in an agitated state, and called almost constantly for the "Blessed Virgin." It was impossible to gain her attention for a moment.

July 18th, morning.—I was unable last evening, by the artificial light, to determine the amount of jaundice. Patient continued to walk the floor, and with clasped hands and upturned face, kept up a constant wail for the "Virgin Mary," until a late hour when she was given a sedative of morphia and chloral. She refused food, but before going to sleep asked for and drank a glass of water. After taking sedative she slept six or seven hours. This morning patient is less agitated and inclined to be dull. She refuses food and drink, and will not permit pulse or temperature to be taken; still impossible to gain any information from her concerning her illness.

18th, evening.—Continues to refuse food and drink. Dulness has given place to stupor, but she continues to offer forcible resistance to every effort made to examine her. Has passed urine several times, but bowels have not moved since admission. Is given a purgative. Jaundice is well marked everywhere, and has decidedly increased since admission, but still is not at all intense.

July 19th, morning.—Patient was very quiet all night. Bowels moved three times. Jaundice increasing. Stupor has become semi-coma. Makes slight efforts at emesis. Pupils equally dilated. Am able this morning, for the first time, to make physical examination, and find that patient has an easily reducible right inguinal hernia, for which she has been wearing a truss, and which appears to have given her no trouble. The area of liver dulness is somewhat diminished. Temperature, 98⅘°. Pulse, 110 and irregular in time and volume. Respirations, 30. Ten ounces of urine drawn with catheter; and is found to be deeply stained with bile, and to contain a large amount of albumen and numerous granular casts from kidneys. Patient gives evidence of pain when pressure is made over liver.

19th, evening.—Pulse, 90 and very feeble. Temperature, 99°. Respirations, 38 and irregular. Pupils largely dilated, but partially react to light. Coma is profound. Efforts at vomiting have continued and grown more frequent, but patient has vomited nothing. Slight hemorrhage from uterus.

July 20th.—Patient died at 12.5 this morning. She gave no signs of spasm at any time.

To-day I gained the following from patient's sister:

Deceased had variola when five years old. The attack was light, and recovery complete. Since then she has not been sick enough at any time to be confined to bed, until her last illness. Two months ago deceased went to the country with the family she has been employed by for some time past, and expected to remain until October, but on the 14th, three days previous to her admission to the asylum, she made her appearance at the residence of her sister, in Brooklyn, about eight o'clock in the evening. She greeted her sister's family as usual, and after talking quietly for a while about her visit to the country, complained of feeling tired, and said that she would retire. When offered food, she declined, saying that she did not feel hungry. Up to this point deceased showed nothing peculiar, so far as noticed by my informant, but on being asked why she had returned so much earlier than was expected, she declared that her mistress had treated her so unkindly that she could not live with her longer, and that the cook had poisoned her. My informant discredits the story about the unkindness of the mistress, as deceased had lived with her for several years, and was very much liked. She declares, however, her belief in the story of the poisoning, and says she has employed a detective to go to the country and work up evidence on which she can prosecute the said cook.

Informant says that after retiring, deceased remained in bed and was quiet but sleepless until about eleven o'clock, when she suddenly sprang from the bed, threw herself upon her knees, and called loudly for the "Virgin" to save her, saying she was lost, lost! She could not be induced to return to bed, and remained in this agitated state until the arrival of the physician at ten next morning. She was then put to bed and kept there by the use of morphia, chloroform, and mechanical restraint, until removed to the asylum. While at her sister's she neither ate nor drank any thing: passed urine several times but bowels were not moved.

My informant further states that patient began menstruating at the age of sixteen.

Informant says she has five unmarried sisters living, who, she claims, are in every way normal. She also denies that there is any disposition to neurotic trouble in her family. She is accompanied by a sister whose physiognomy and behavior gave unmistakable evidence of a defective nervous system.

When I informed these two sisters that an autopsy had been made on deceased, they both had prompt outbursts of hysteria and heaped abuse upon me unsparingly. When, finally, it was suggested to them that they should be obliged to me for having made the post-mortem, and thus removing the disgrace that would have attached to the family name (had not the autopsy decisively revealed that deceased was not insane), removed a serious impediment to the marriage of the remaining sisters, my visitors at once became as apologetic as they had been abusive.

It is not believed that the inherited neurotic tendency, which I believe the patient had, predisposed particularly to acute atrophy, but it may account for the early appearance and violence of the delirium.

Post-Mortem Appearances.—Autopsy eleven hours after death. Post-mortem change very slight ; rigor mortis entirely absent ; body fairly well nourished. Skin of head, neck, abdomen, and arms considerably jaundiced, as is the sclerotic ; that of legs slightly so. Skull cap normal. Vessel of dura possibly a little engorged. Pia normal, as also is the cerebral substance. Lateral ventricles contain a moderately increased amount of yellow-stained fluid. Numerous small ecchymoses under visceral pleura of both lungs, and two small hemorrhagic infarctions in anterior portion of upper lobe of left lung. Yellow-stained mucus in bronchi.

Heart is of normal size ; valves are in good order ; large black soft clot fills right auricle and ventricle, and a smaller one of same description partially fills left ventricle. There are numerous small ecchymotic spots to be seen on the visceral layer of the pericardium ; there are numerous ecchymoses in omentum and mesentery, but none to be seen on peritoneum ; mesenteric glands not enlarged. Mucous membrane of the intestine is anæmic and of a dirty grayish appearance ; that of stomach shows no changes apparent to the naked eye. There is no solid fœcal matter in intestines, but both the large and small intestine contain a small amount of a chalky-colored mixture of mucus and liquid fæces. Several Peyer's patches near the ileo-cœcal valve present the shaven-beard appearance very slightly. Spleen is strongly fixed in the hollow of the diaphragm by old adhesions, is enlarged by one half, is soft, and of a dark reddish-brown appearance. The kidneys are about normal in size, markings indistinct in places, capsules removed with some difficulty, tearing the cortex as it comes away. Uterus is normal in size ; four small cysts in left broad ligament ; a cyst, size of large bean, and containing a clear serous fluid, is found near the centre of right ovary. Mucous membrane at fundus is swollen and congested. No recently ruptured Graafian follicles to account for uterine hemorrhage on last day of life. Os is slightly eroded.

The liver is collapsed and partly covered by the small intestines; it weighs thirty-eight ounces, is soft, flabby, surface much puckered, and margin very sharp.

The dimensions of the organ are diminished in every direction, the left lobe being especially thin.

The gall bladder contains about an ounce of thick dark bile, and its communication with the intestine is unobstructed. The tissue of the liver is flabby and moist, and, except a part of the right lobe, is almost bloodless; the left lobe is of almost a uniform ochre-yellow color, but even here the vessels surrounding the lobules can be seen to be slightly injected, while the centre of the lobule is of a yellow or grayish-yellow color; near the thin margin of this lobe the capillary injection entirely disappears. In the right lobe the process is not so far advanced; this lobe, in its thickest portions, is congested at places, and near the centre of this lobe are several moderately large extravasations of blood into the liver tissue; these spots are so soft as to scarcely bear the most careful handling, and here the tissues seem completely disorganized.

EDITORIAL DEPARTMENT.

THE BATTEY-TAIT OPERATION.

THERE is no subject which has recently come up in medicine that is more interesting or important, than the question of the legitimacy and scope of the operation for the removal of the ovaries. It is now twelve years since Prof. Hegar in Germany, Mr. Lawson Tait in England, and Dr. Robert Battey in the United States—almost simultaneously each did his first operation. Dr. Battey was the first to publish his case (which was also the first successful one) under the title of "Normal Ovariotomy," a somewhat unfortunate name, which was soon abandoned for the one suggested by the late Dr. J. Marion Sims of "Battey's Operation."

Various other titles have been suggested for the proceeding, as "Castration of Women," "Spaying," "Oöphorectomy," "Removal of the Uterine Appendages," "Ovariotomy." This diversity of titles is fairly indicative of the diversity of principles underlying the various operations. The term ovariotomy would probably be the most acceptable, if it were not that by general usage it has come to be restricted to the removal of tumors of the ovary.

Such being the case I shall not attempt to add another to the too numerous titles already used, but shall briefly consider the various modifications of the procedure as the Battey-Tait operation. At the meeting of the International Medical Congress in London, Dr. Battey reported a table of nearly two hundred completed operations which had been performed up to May, 1880.

It is certainly not extravagant to estimate the published cases since then, up to date, as from two to three times that number. One cannot help being struck with the fact, that some great operators are conspicuous by their absence from the published lists. Thus where Mr. Tait does a hundred operations, Spencer Wells only does one or two. Yet these two distinguished men must see about the same class of cases, indeed we know that in some instances they see the same individual patients, for Mr. Tait takes particular delight, apparently, in telling us that in many of his cases an operation had been previously refused in London. I think Dr. Keith has only done the Battey-Tait operation once or twice.

These facts speak volumes ; they may be considered as fairly indicative of the estimation the operation is held in by different competent observers.

It would seem that we might now be in a position to define the indications for the operation, and to settle the question of its legitimacy and scope. But such is very far from being the case, and it is certainly remarkable that a capital operation has been done so many times within such a short period, on such vague grounds and with such wild hopes. There has been no dearth of opposition to the operation, but it has been chiefly active, either in the form of shrieks about the iniquity of spaying women, or in impugning the motives and veracity of Mr. Lawson Tait. On the other hand, the advocates of the operation have manifested a certain fervor of statement and belief much more suggestive of a religious revival than a scientific discussion.

The operation has been hailed as a cure-all for the woes of womankind, and the more desperate the case with the more enthusiasm has its performance been urged. Again, numbers of cases have been reported cured, to which grave exception must be taken.

There has been far too little expression of opinion on the subject from the profession at large. Perhaps it is too soon to expect that impartial summing up of the reasons for and against, which has to come from outsiders, not from operators simply, before the operation takes its due rank among the settled procedures in surgery. But a few more discussions such as that held at the Am. Gyn. Soc. meeting in 1880, would do more toward such a settlement of the questions at issue, than scores of cases as now published.

The necessity of having-well defined grounds for operating is all the more pronounced because of the great dangers inherent in the operation to life and happiness, and the fact that it is usually done in the comparatively young. Thus the average age in Dr. Battey's table, calculated from 132 patients, is under 32 years. Of the actual dangers to life far too little has been said. Mr. Lawson Tait in especial has been singularly rash in minimizing the risks, and he has probably done a great deal of harm in encouraging men to undertake a so-called "easy operation," which too often has ended fatally for the patient. Mr. Tait's own cases, and those of Mr. Savage, are marvellous, and they go far to justify them in personally operating—but the same does not obtain among their followers. I think, moreover, we shall find later on, that the greater the need and justification there is for the operation, the more dangerous it is apt to be. In Dr. Battey's table the mortality is eighteen per cent., and I do not think from a tabulation I have made of the cases since then that the general average has been lowered.

Nor is the question of mortality the only one we have to take into consideration—for turning again to Dr. Battey's table, and taking all the cases in which the ultimate results and which recovered are given, we find that 65% are put down as "cured," while 22% were "benefited," and 11% were "not benefited," the remainder not stated. In making up our minds as to the dangers and benefits of the operation, we must not forget that a certain number who recover are not cured. Taking Dr. Battey's own very fair analysis of the results as a basis, we may conclude that of ten women who are operated on for all the causes that the Battey-Tait operation has been done to relieve, he claims that five are "cured," two are "benefited," while of the remaining three one recovers, but "not benefited," while the other two die.

Mr. Lawson Tait's claims for the operation, based mainly on his own experience, are very much more sanguine.

I think it will be hardly questioned that, taken right through, the average Battey-Tait operation will be found more difficult to perform, and more dangerous to life, than the average ovariotomy. Such at least is the opinion of such men as Spencer Wells and Dr. Keith. The method of operating which was for some time under dispute, may be now considered as settled. In Dr. Battey's first ten cases, which were largely tentative and experimental, he used the vaginal method, but it is now conceded by him, that in many cases it is wholly inapplicable, and it certainly has never given the same brilliant series of results that the abdominal incision has in Mr. Tait's hands. Several other modifications of the operation introduced by Mr. Tait are of great importance. One of these is his peculiar manner of ligating the pedicle by means of the "Staffordshire knot." I am quite sure that this most valuable improvement is by no means properly understood, when in the last edition of his book Spencer Wells makes no mention of it, and one of our own most distinguished surgeons ties an entirely different ligature and then blames "Tait's knot" for not controlling the bleeding. Another most important addition by Mr. Tait is his removal of the Fallopian tubes. I can hardly share his belief in the great importance of these organs in health—a point which he asserts with great positiveness, but on which he adduces no evidence,—but there can be no question of the great mischief they may produce when diseased. More especially in the condition of pyo-salpinx their presence is fraught with great danger. In cases of hydro-salpinx, however, I question the justifiability of resorting to their removal, or the extreme difficulty or inutility of tapping, as he asserts. Another procedure which Mr. Tait has resorted to, brings up an even more serious question :—he has in certain cases removed the tubes and left the ovaries, which were healthy, behind. It would seem as if he thereby rendered the women liable to recurrent attacks of pelvic hematocele and peritonitis, a difficulty by no means met by his mere statement that he believes that the vast majority of ripe ova fall into the peritoneal cavity any way, without producing any mischief. The only other surgeon I know of that has similarly removed the oviducts, leaving the glands behind, is Mr. Wood, of Cincinnati. He did so (Am. Gyn. Trans., 1880) on the declared ground that he thought the woman would be happier, would derive more sexual pleasure than if they were removed. And this brings us to the questions: What are the effects of removal of ovaries and tubes, 1st, on the sexual system; 2d, on the general system? On the answer to these questions will depend to a considerable extent the decision as to the morality of the operation, but on these points the evidence is singularly defective.

It has been pointed out very ingeniously by Dr. Battey, that certain of the discrepancies are to be accounted for by the fact that a portion of the ovarian glandular tissue has been left behind, and so has kept up for a time at least a remnant of functional activity.

He instances a case of his own in which this explanation held good, and there is one case, I believe, on record in which pregnancy occurred subsequent to a so-called Battey's operation. But excluding the rare cases in which a third ovary is present, and taking only those in which the ovaries have been completely removed, what are the effects on the sexual system? The first and most important result is of course the absolute arrest of ovulation, and therefore the possibility of child-bearing, and on this all are agreed. Next comes the question, What is the effect on menstruation—is this at once and completely arrested? The answers are not unanimous, because many cases have been reported of the continuance for a longer or shorter period of a monthly flow. It is to account for these anomalous cases that Dr. Battey suggests a remnant of ovarian glandular tissue or a third ovary, while Mr. Tait boldly alleges that continued menstruation is due to non-removal of the tubes, and asserts that if these are completely removed, the function is at once definitely arrested. I must confess I do not think either explanation is entirely satisfactory, for I recall at least one case in which all the ovarian tissues were removed with the tubes close up to their emergence from the uterus, in which such monthly flow reappeared. It seems more physiological to suppose that the habit impressed by the ovarian stimulus (ovulation) on the nervous system, leading to menstruation, may to a certain extent remain and only gradually die out. A formed habit may continue its periodical effects, though the present cause is removed, and in consonance with this view we should expect to find that the more completely this habit has been formed, the more perfected and ingrafted on the woman's system the functions of ovulation and menstruation have been, the more likely would such after-menstruation be to continue. In other words, the woman of thirty who has borne children, and becomes the subject of the Battey-Tait operation, will be more likely to continue the after-effort at menstruation than her virgin sister of eighteen who has been similarly treated. On the structure of the uterus the effect would seem to be to bring about the same diminution in size which follows the physiological change of life. But to this there may be exceptions to be noted below.

The question of the influence of this operation on the sexual appetite is one that has been eagerly canvassed, and is one of importance and delicacy. From the nature of the subject the question is one on which the evidence is deficient. It may be at once readily granted that the operation will restore a certain number of women to the possibility of fulfilling their marital functions by removing a source of pain—such as an inflamed and prolapsed ovary which had been a bar to connection before. But the question as to the effect on the woman herself is quite undecided. Dr. Thomas has reported a well-known case, in which the woman, passive in her sexual relations before the removal of both ovaries, became aggressive afterward. The question becomes an important practical one in those cases where it is proposed to do the Battey-Tait operation for nymphomania or incorrigible masturbation. Such operations have been done, although Dr. Battey is on record as strongly opposed to them, but the results obtained have not been satisfactory. The time element in this relation seems important, and its consideration unduly neglected, and the remarks already made with regard to the question of after menstruation as dependent on formed habit, apply here to a still greater extent. Judging from the analogy offered in men in the case of eunuchs, the removal of the testicles has not proved sufficient to arrest sexual desire, and consequently in the East the penis is also amputated. It would be natural to expect that while the Battey-Tait operation might prove curative in very recent cases of masturbation, it would not in chronic cases or in nymphomania.

Turning to the ordinary cases for which the operation is done, —not for these loathsome moral diseases,—it is probable that sexual desire is not destroyed at once, though possibly, as a rule, it is diminished, and undergoes a somewhat premature decay. To settle this point we need observations extending over a longer time after the operation than most of the cases so far published.

Turning to the effects the operation has on the general system, we are still more wanting in evidence. Here the analogy with the male is not apt to hold so good, because in the man the alterative influence of puberty is much greater, in its outward manifestations at least, than in the woman. Such is the case, for example, with the voice—for the girl's and the woman's voices are at least an octave nearer together than the boy's and the man's. Similarly, the appearance of hair is much more marked in the male than the female. All of this would lead us to infer that the general change following the Battey-Tait operation would not appear as great as that following castration in man. The changes that do occur are less in the nature of a reversion to the type of girlish childhood than in the approximation to a masculine type. The closest analogy is to be found in cases of atrophy or non-development of the ovaries—Nature's Battey-Tait operation. In these we find the absence of ovulation and menstruation, the tendency to hair-growth on the face, and the excessive development of adipose tissue; but neither in the voice nor in the contour of the form are there any indications to show the complete abeyance of ovarian function. The chapter in physiology that this question opens up is a most interesting one, and cases carefully observed after the Battey-Tait operation for several years, will furnish the data that will enable us to fill in its at present meagre and blank pages.

If, now, we turn to an analysis of the various published cases of the Battey-Tait operation, we find that they can be arranged under five different heads, according to the indications for which the operations were done. It has been proposed to remove the ovaries (as suggested by Hegar and Battey), or the ovaries and tubes (as advocated by Tait):

I. For the arrest of the menstrual molimen where this is inflicting grave injury on the patient because unrelieved by monthly flow, due to:

(*a*) Congenital absence, or quite rudimentary development, of uterus and vagina.

(*b*) Traumatism leading to obliteration of the utero-vaginal canal.

II. For the relief of organic disease, otherwise incurable, of the ovaries.

III. For the relief of certain nervous symptoms—especially pain—the ovaries themselves presenting no special or recognizable evidence of organic disease.

IV. For the arrest of: (*a*) uterine hemorrhage, and (*b*) the growth of uterine tumors.

V. For the cure of certain uterine diseases and salpingitis.

There can be little question that, in the cases which truly belong to the first class, the operation is entirely justifiable. Fortunately, they are not numerous, but Dr. Battey cites two,—one belonging to each subdivision.

In the congenital cases, the best proof of the necessity for operative interference is found in the fact of the hopelessness of all other modes of treatment. Most of the older text-books allude to these cases merely, and pass them over with the remark that nothing can be done for them. The only suggestion that has proved of any efficacy—and that not of very much—was the plan proposed by Dr. Thomas. In view of the occasional occurrence of vicarious menstruation, he suggested bleeding from the arm at each regular monthly period. If menstruation consisted simply in the loss of so many drachms and ounces of blood, this proceeding would, no doubt, meet every requirement, and its comparative safety would, to a certain degree, compensate for the inconvenience of systematically carrying it into execution.

But the phenomena, local and constitutional, of menstruation, are very much more complex than that; and where grave mischief was taking place, as in Dr. Battey's case, the radical operation would offer the only cure, or even hope of benefit.

In the second set of cases, in which the perviousness of the utero-vaginal canal is obliterated by some traumatism—usually due to parturition—the first question to be setttled is the possibility of restoring the channel itself by an operation.

This may be an extremely difficult matter to decide, but unless it is fairly evident that the obstructing cicatrix is more in the nature of a circular membrane of not too great depth, and not a long impermeable band, reaching high up and involving the utero-vaginal juncture and the peritoneal cul-de-sac, it would seem far preferable to take the risk of the Battey-Tait operation, which is a known quantity, to the unknown dangers of an elaborate dissection. The mortality from the operation in this class of cases should be very low, as the seat of malformation lies not in the ovaries but the uterus. Several cases of the traumatic class where the operation would have been applicable have been published, and it has been done successfully. I know of one case in this city in which it would have been in order, where the reckless use of the forceps before dilatation of the os was complete, led to complete circular rupture of the uterine neck and vagina. Some weeks subsequently—for the patient survived this extraordinary accident—on making a vaginal examination a closed canal was found, the uterus being unappreciable by the finger.

In these cases, therefore, both of the congenital and traumatic forms, Dr. Battey has done a good thing in pointing out their only method of cure.

II. Cases of organic disease, otherwise incurable, of the ovaries.

This class, I think, comprises most of the patients in whom it is justifiable to do this operation. Given a patient with advanced ovarian disease, for the relief of which all other rational methods of treatment have been faithfully tried and in vain; whose health is being undermined, and her happiness and usefulness in the community destroyed, then the Battey-Tait operation is entirely justifiable. This is not the time or place to consider all the forms of disease of the ovary that come under this class, or the diagnostic signs by which we recognize them, but there are one or two special points worth considering, as they have not generally received their due share of attention. The first of these is the essentialness of objective signs and not merely subjective symptoms in establishing a diagnosis. It may sound like a truism to make a bald statement like that, but more mistakes are made, more unjustifiable operations undertaken, and more lives sacrificed because this essential point is overlooked, than can be estimated. We have to be continually on our guard against the deceptions, unintentional often, of the patient's feelings. Take, for instance the symptom of pain. In nine cases out of ten, a woman will refer *any* abdominal pain to the ovaries. Now if the glands are healthy, we will find, on bimanual pressure, that the sensation produced is *not* painful, but merely the characteristic, distressed, sickish feeling. As a matter of fact the ovaries are remarkably insensitive in health, and consequently exquisite tenderness and shrinking on touch or pressure, excluding the hysterical element, is a most valuable sign in disease.

The importance of displacement, prolapsus of the ovaries, has not received the recognition it deserves. The anterior displacement is rare, but where it is producing serious mischief the Battey-Tait operation offers the only rational hope of cure.

The posterior displacement, in various degrees of severity, is very much more common, but, fortunately, it is not generally so unamenable to treatment.

Unless the prolapsus is of an extreme degree, we may hope to do much by mechanical means. Where, however, an ovary lies so low down as to be the first thing encountered by the finger after entering the vagina, especially if it is bound down by adhesions or by a heavy and retroverted uterus, the chances of any thing short of the radical operation effecting a cure, are very slight. It is remarkable how quickly an ovary misplaced to this degree will undergo pathological changes. It is probable that it was in a state of acute or chronic congestion in the first place, either giving rise to or accompanying the displacement. These ovaries so soon become the seat of cystic degeneration, that probably this pathological lesion must be, in large part, due to perverted functional activity. The extent to which adhesions are present in these cases of ovarian disease is a very important question. I believe it is, broadly speaking, true, that the more extensively the ovaries are matted up in the products of an old peritonitis, the greater will be the necessity for the operation. This suggests the unpleasant reflection, that the greater danger is there of it being fatal If it should ever become recognized as sound practice to follow the example of Mr. Lawson Tait to make the abdominal incision and drain cases of pelvic abcess and peritonitis, it is probable that we should save many patients from the necessity subsequently of having their ovaries removed.

A large number of cases of ovarian disease are undoubtedly the secondary results of effusion of the products of inflammation into the pelvis, in the meshes of which the glands become entangled and subsequently diseased. It is becoming more and more recognized as sound practice that in cases of pus in the pleural cavity, drainage is the best treatment alike for the acute febrile condition and the avoidance of future pulmonary disease consequent on collapse of the lung and extensive pleuritic adhesions. It is by no means impossible that it may yet prove feasible to carefully liberate the ovaries from bands of adhesion, and then get rid of the products of inflammation by careful drainage.

In any event the line of thought Mr. Tait has opened up by his daring performances is a most suggestive one. And in the treatment of otherwise incurable ovarian disease by removal of the organs, he has been as sound in principle as he has been successful in practice.

III. The third class of cases, for the cure of which the Battey-Tait operation has been proposed and has been done, embraces the nervous ones. And here it may be well to again emphasize, that by this class is not meant those cases in which nervous symptoms play an important part simply, for that would practically include all five divisions. But I mean those cases in which the nervous disturbances constitute *the* disease; in which the operation is done for their relief, and not for a known diseased condition of the ovaries themselves.

It is about this class of cases that the hardest fight has raged between the opponents and the advocates of the procedure. And it must be at once acknowledged that it is in this class of cases that the greatest room for doubt arises. When a man removes an ovary which has undergone marked pathological change, and yet fails to relieve his patient, his after-thoughts are no doubt bitter and sad; but he has at least the satisfaction of knowing that he has removed an organ which could not be of any further service to its possessor, and might be a future source of danger. But no such consolation awaits him who removes what are apparently healthy glands, and the patient's sufferings are not abated. If he is honest, he must know that he has inflicted a positive harm on a fellow-being, and he may well ask himself if he was justified in the process of reasoning which has led to so deplorable a result.

It may well make one pause in accepting such a responsibility where the conclusion is so largely based—not on physical signs—but on more or less rational inferences. The whole subject of reflex influence is so obscure in its physiology that it is simply with a feeling of amazement that one reads on what mere guesses this most hazardous operation is undertaken. In this branch of the subject we should expect that the neurologists' aid would be valuable, yet in how many cases in which the ovaries are removed for "pain" or "epilepsy" or "insanity," is their counsel sought? In these cases accuracy of diagnosis is pre-eminently needed, to justify the morality of the proceeding. These remarks are neither gratuitous nor uncalled for; case after case could be cited in which the operation has been undertaken on the most inaccurate diagnostic grounds. It is at all times a dangerous proceeding to operate for the relief of reflex symptoms, which are in large part entirely subjective; but if it is ever done, one would think the first point to be settled is the credibility of the witness. Now it is quite notorious how largely the hysterical element enters into many of these candidates for the Battey-Tait operation.

I confess I cannot see how that element can be satisfactorily excluded in patients sent to some noted operator from a distance, except by prolonged and minute personal observation. It may be said that the doctor who sent the case to the surgeon had the patient for a long time under observation. Grant it, and in return I would like to ask what man is justified in doing a capital operation like this on another man's diagnosis? Many of the cases thus hastily operated on, are as hastily reported cured, quite honestly, I do not doubt, but they are singularly fallacious. It is well known that profound impression made on the nervous system by such a grave operation will often temporarily modify and arrest the most deep-seated morbid processes, like epilepsy, and yet the effect is *not* permanent. But the sanguine operator is quite sure the patient is cured, for was she not quite well when he dismissed her?

In considering the general question of the morality of the operation, I drew attention to the fact that most of the subjects of it are comparatively young. That fact, and the civil condition of the patient, have an important application with reference to these nervous cases. A certain number are unmarried women, while others, the married, are unhappily so, as regards their sexual relations. In these cases the extent to which their symptoms are due to unsatisfied sexual needs has to be carefully considered. If they were condemned like the anchorites of the desert to a perpetual celibacy, it would be, no doubt, eminently well worth considering whether the removal of their reproductive glands was not in order. But such is not the case in the great majority of cases, and it may be safely affirmed that, where one may be relieved by, if she survive, the Battey-Tait operation, at least twenty will be cured by a congenial marriage and the bearing of children. There is another important element in the consideration of this class, and that is, that so many of them are addicted to the abuse of stimulants and narcotics. Mr. Lawson Tait has borne testimony to the harm many of his countrywomen are doing themselves by their abuse of alcohol to relieve pelvic pain, and he seems quite unconscious of how much that admission may invalidate their testimony when they come to see him to have their ovaries removed. Dr. Matthews Duncan stated it as his opinion that the majority of this class were addicted to the habitual or periodic use of opium. And who can estimate, in addition, how much the bromides, chloral, patent nostrums, "vegetable mixtures," "elixirs," and the nameless compounds which are so accessible now to women, have to do not only in the perpetuation, but also in the creation, of many of these symptoms? Turning now to the individual symptoms, for the relief of which the operation has been proposed, they may be grouped under four subdivisions:

(*a*) Pain.
(*b*) Epilepsy and hystero-epilepsy.
(*c*) Insanity.
(*d*) Perversion of the sexual instinct.

(*a*) "Pain referred to the ovary," is a very frequent diagnosis in the literature of the subject, but if it is more closely examined it almost invariably should be, "pain referred to the pelvis." It is hard to see how a perfectly unmistakable diagnosis of neuralgia of the ovary can be made. Even supposing the hysterical element and the use of narcotics can be excluded, and the honesty of the witness is beyond cavil, how are we to be quite sure of our diagnosis? If it is unilateral pain, are we to remove one ovary and leave the other behind? That has been tried, but not with brilliant success. If the pain is worse at the menstrual epoch, what pain, pelvic or other, in the woman's body is not worse at that period? If we are to be logical, why should we not remove the ovaries in those women who suffer from sick headache (migraine) every month? A far closer analogy can be traced between such megrim and menstruation than between it and ordinary ovarian neuralgia. In the case of megrim, we are supposing, the sick headache only appears at the monthly period, while the ovarian pain is present at other times and only suffers an exacerbation then. But granting the accuracy of the diagnosis, that you can be as sure of a neuralgia being located in the ovarian nerve as you can be of one in the second division of the trigeminus, what are your chances of curing your patient by removing the peripheral expansion of your nerve? This question may, perhaps, be best answered by asking in how many neuralgias elsewhere it is deemed right to exsect a nerve? Has it not been tried time and time again, for instance, for tic-douleureux without relieving the patient? Is it not true that, even where a neuralgia starts as a purely local irritation, it leads to structural changes in the related nerve-centres, which, once established, are quite out of reach of the scalpel? Long before it would be deemed justifiable by the most radical gynæcologists to remove the ovaries, such a central nervous impression has been made.

The argument that has been advanced, that the nerve-centre, like the nerve fibre, will undergo secondary degeneration, and so gradually end it, is quite too theoretical to need discussion. How many neuralgias, however, are such purely local affairs? I think it is true that the vast majority of true neuralgias, not mere temporary discomforts,—but acute pain which comes and comes again in a particular nerve or nerves,—are the local expression of a constitutional condition. It is only on this pathology, that neuralgia is the expression of nervous malnutrition, that the immense influence of heredity in its causation can be accounted for. If it is a purely peripheral irritation, how is it that we find it in families alternating with epilepsy and even phthisis? No doubt the local expression, the localization of the pain in a particular nerve trunk, is due to some weakness or irritation at the periphery in many cases, but in a healthy condition of the central nervous system such an irritation would never become a neuralgia.

The results of treatment corroborate this pathology, for the various remedies of value in neuralgia—counter-irritation, galvanism, nerve tonics as phosphorus—all act by improving the nutrition of the nervous structures.

The instance of the irritable and painful stump, which is cured by section of the offending nerve fibres, so often cited in justification of the Battey-Tait operation done for pain, is quite beside the mark, and entirely overlooks the distinction between an injury and a disease.

This class of cases are those particularly apt to be benefited by the systematic treatment formulated by Weir Mitchell, and this plan bears a curious inverse relation in value to the Battey-Tait procedure. While the Weir-Mitchell system is contra-indicated in the inflammatory and organic type of ovarian disease, it is of great value in the neuralgic and functional perversions, and the exact reverse, I think, holds good of the Battey-Tait operation. If the above views are correct, as I believe them to be, we must unhesitatingly conclude that removal of the ovaries for the relief of pain simply, for the cure of neuralgia is, entirely unjustifiable.

(*b*) Epilepsy and hystero-epilepsy.

It is no new thing—the proposal to cure epilepsy by an operation. Some time ago, but not so long that we can afford to be too contemptuous in speaking of it—our medical forefathers trephined the skull to allow the devils to escape. Perhaps our descendants may in turn be inclined to smile, that in our generation it should be seriously advised to remove the ovaries for the same disease. It is true we do not any longer talk of devils in this connection, but it is a question whether many of the so-called reflex symptoms are not as much unknown quantities.

Since Brown-Séquard's researches on epilepsy, comparatively few operations for its cure have been proposed, except when a well-marked aura was present. But they have not been such as to warrant much belief in their value, even in the simplest cases, where the aura travelled on easily ascertained paths, as along the nerves of the arm to the hand—how much more chimerical is the hope of benefit when the aura is referred to an organ like the ovary, under the influence of a most complex nervous supply from both the cerebro-spinal and sympathetic systems. We know so little of the essential nature of epilepsy, that at most the advocates of the value of the Battey-Tait operation for its cure would have to base their claims on clinical evidence. And if we examine the published cases, there is not a single one on record of long-enough duration to warrant it being pronounced a cure. No better witness to the truth of this assertion can be found than Mr. Tait himself, and this is what he says about a case that is ordinarily put down as a "cure": "The second case I operated upon was a girl, aged eighteen, who had been imbecile from birth, and who had developed the most violent menstrual epilepsy from the time of the molimen. * * * I operated on May 9, 1880, with the result of completely arresting menstruation and *abolishing the epilepsy*. * * * She gives slight indications of an increased noisiness and loquacity at the time at which her periods should occur and occasionally at these times she has an attack of *petit mal*.

The words I have italicized are significant, and it is certainly an extraordinary conception of nervous pathology which can allow of the statement that the epilepsy is abolished while the attacks of petit mal continue!

Another case I have heard of in which the ovaries were removed for epilepsy with an aura said to be ovarian, but which turned out to be, after the operation, from the epigastric region! The Battey-Tait operation for the cure of epilepsy, then, must be absolutely condemned. In hystero-epilepsy the question is more undecided. I imagine that the less marked the epileptic element is, and the more pronounced the hysteroid, of this vague disease, the more is to be hoped from the operation. It has occurred to me that there is a point which might aid us very materially in deciding as to the probable benefit from the operation. Some years ago M. Charcot drew attention to the fact that in certain hystero-epileptics the fits could be arrested or modified by strong pressure over the ovaries. Now in certain cases this does not obtain, and no amount of pressure exerts any appreciable influence, but in those in whom it *does*, it is likely at least that the Battey-Tait operation would effect permanently what pressure did temporarily.

This whole subject is so vague and ill-defined that the justifiability of the operation is, as yet, not established.

(*c*) Insanity.—Several cases are on record in which removal of the ovaries was followed by improved mental capacity, but the statements are vague.

On the other hand, Dr. Thomas has had two cases (quoted by Dr. Pallen, Int. Med. Congress), in which the development of insanity followed the removal of the ovaries.

A condition that one might *a priori* think to be peculiarly likely to be benefited by the Battey-Tait operation, is menstrual mania, but the following case shows that, except as a last resort, it is entirely unjustifiable. Dr. Fordyce Barker related a case (Am. Gyn. Soc., 1880,) which is so important that I will quote a portion of it : "At the occurrence of each menstrual period she had acute mania for two or three days, and in the intervals between her menstrual periods, she was morbid in her tastes, her feelings, and her sympathies, and she was in every way a source of great misery to herself and unhappiness to her family. I placed the patient upon bromide of potash three or four days previous to the occurrence of the menstrual period and during menstruation. As soon as the symptoms of acute mania manifested themselves, I directed that she should receive one twentieth of a grain of hyoscyamia, to be repeated every two hours until she became quiet. Subsequently the quantity of hyoscyamia was one sixth of a grain three times a day. It has now been nearly eighteen months since this plan of treatment was adopted, and menstruation has become much easier than formerly, and the change in her moral and mental condition has been most remarkable. By the use of hyoscyamia during menstruation she becomes perfectly quiet and tranquil. I have pushed the drug to the production of its constitutional effects in some degree, and the result has been most happy, but generally not more than three or four doses are required. Her morbid likes and dislikes have all subsided, and the melancholia which existed in the intervals between menstruation has entirely disappeared. She still continues to take hyoscyamia at the time of menstruation, now only one tenth of a grain three times a day."

I think this case contains an important warning to those who so eagerly resort to this operation. I do not think that Mr. Tait would have hesitated to operate on this patient, and the like is perhaps true of Dr. Battey ; and yet even had the operation terminated in the recovery and cure of the patient, I should say it was entirely unjustifiable, for she was practically cured by Dr. Barker, without subjecting her to the terrible risks of a fatal result. I do not know of any attempt to formulate any principle with regard to these cases of insanity and the probable effect of removal of the ovaries on their mental condition. But I think some conclusions reached by Dr. Skene some years ago, in the course of extended study at the King's Co. Insane Asylum, on the relations between gynecology and insanity, are applicable here (see ARCHIVES, Feb., 1880) :

" Well-developed insanity with impaired general nutrition causes suppression of the functions of the sexual organs. The influence of insanity on this class of diseases (functional diseases) is most favorable. It may be stated fairly that such diseases disappear on the occurrence of mental alienation. Organic diseases of the sexual system exercise a most important influence in causing insanity, and tend to retard recovery from it. A careful consideration of this subject has led to the conclusion that *acute* affections of the brain and nervous system, that are wholly due originally to disease of the sexual organs, will be relieved in a large majority of cases by curing the primary affection. The effects of treatment of the disease of the sexual organs will be in proportion to the duration and severity of the mental derangement. In subacute mania, caused or aggravated by disease of the sexual organs, marked benefit or prompt recovery may be expected to follow the cure of pelvic disease. On the other hand, chronic mania, associated with the disease of the sexual organs, will often remain unchanged after the local disease has been relieved."

In applying these general principles to the particular question of the value of the Battey-Tait operation in the relief of insanity, we are forced to the conclusion that it depends mainly on the condition of the ovaries.

If these are affected merely functionally, the operation is wholly out of the question. If, on the other hand, there is evidence of structural disease—organic changes in the ovary which are leading to mental impairment,—then the operation is entirely justifiable, and the more promptly it is done the more likely it is to prove curative.

(*d*) But little need be added here to the remarks already made as to the non-justifiability of the operation for cases of perversion of the sexual instinct—self-pollution and nymphomania. For the first of these conditions, if the bad habit is not too strongly formed, if the operation is done early enough, it might prove of benefit, especially if clitoridectomy was done at the same time. But where the habit is fully formed,—where it has led to that peculiarly intractable form of insanity due to masturbation,—it is not likely that the operation would be of the least benefit. It is probable that only in the advanced cases most men would feel justified in advising, or be permitted to do, an operation, and therefore the value of this field for the operation, or the prospect of its being enlarged, is not promising. With regard to nymphomania, such evidence as there is is quite against the justifiability of the operation.

IV. The fourth class of cases in which the Battey-Tait operation has been eagerly championed comprises those in which life is endangered by (*a*) profuse uterine hemorrhage or (*b*) the growth of uterine tumors. Very often the second of these, the presence of large tumors, is mainly dangerous by giving rise to the first, the hemorrhage. But even where there is no tumor present, but where there is profuse uterine bleeding, either at the monthly period or between times, the Battey-Tait operation has been done with the hope of arresting it. It has been argued on two grounds that this would prove curative: the first one, because you would "cut off a portion of the blood-supply of the uterus"; the second one, that "the effect on the nervous system would be such as to bring about the change of life and the consequent diminution in the size, etc., etc., of the uterus."

The first of these arguments needs only a brief consideration. The uterus is mainly supplied by the utero-ovarian arteries, branches of the internal iliacs. After supplying the uterus, they anastomose freely with the branches of the ovarian arteries derived from the aorta. Now, while the ovarian arteries would be tied in the pedicle after a Battey-Tait operation, and so cut off this source of supply to the uterus, the utero-ovarian (anastomosing and ovarian branches) would also be tied, and the whole supply from the internal iliacs would be concentrated on the uterus. If, therefore, the soundness of the procedure rested on the circulatory changes it was supposed to produce, it would be entirely irrational. The second argument, the diminution of the ovarian nervous influence, is more plausible. But the question is by no means so simple as it looks, and its determination involves the further one as to what is the cause of the hemorrhage. It may be well to recall a fact connected with Dr. Battey's first operation, namely, that it was followed by metrostaxis, from which the patient had been quite free before. That serves to show that we must make some distinctions between cases. Again, Dr. Trenholme reported a well-known case of alarming recurrent hemorrhage, due to a myoma, as cured by the Battey-Tait operation, and yet, some months later, Dr. Byford stated at the Am. Gyn. Society that this very case was then under his care, the bleeding having recurred as profusely as before the operation. If we analyze the causes of hemorrhage in these cases, we find them to be of four kinds :

1st.—A diseased condition of the uterine mucous membrane.

2d.—A certain nervous influence reflected from diseased ovaries, leading to too frequent or profuse menstruation.

3d.—The hemorrhagic diathesis.

4th.—The presence of neoplasms.

If the hemorrhage is due to a diseased condition of the mucous membrane of the uterus it would be quite unphysiological to expect to cure it by this operation. Even though the menses should be promptly arrested—of which we cannot be sure—there is no evidence whatever that the diseased process would be ; on the contrary, there is a possibility that it might take on a malignant character. If the hemorrhage is secondary and due to a morbid ovarian influence, then the question resolves itself into one as to the justifiability of operating for the ovarian disease. If it is decided that the ovaries are incurably diseased, and that they come under the second class of our whole subject in whom the operation is justifiable on the grounds given above, then the operation would also be likely to cure the uterine symptoms. But even then it is by no means certain, and it would not be right to operate for the mere relief of a symptom—however serious. In the cases where the excessive menstruation is due to the hemorrhagic diathesis it is evident that the less coagulability the blood has—the more dangerous the Battey-Tait operation itself becomes, and the more the risk of secondary hemorrhage. It therefore resolves itself into the question, is the danger of the patient's dying before the menopause from the capillaries of the uterus greater than the danger of sudden death from secondary bleeding from the ligated vessels in abdominal cavity and walls *plus* the other dangers of the operation. Put in that way we think few would care to take the responsibility of the operation, but would rather trust to the ordinary and safer methods, such as the tampon, the use of astringents, and styptics, and, if need be, the cautery. When the hemorrhage is due to the growth of neoplasms—the possibility of checking it is bound up with the question of the arrest of their growth by the Battey-Tait operation. It is not out of place to remark here how little is said by our English brethren of removing the offending growths by the vagina. In a paper by Mr. Knowlsey Thornton (Am. Gyn. Soc., 1882), he considered "The Relative Value of Hysterectomy and Complete Removal of the Uterine Appendages for the Cure of Fibroids of the Uterus," as if these were the only alternatives in all cases. Dr. Thomas pointed out in the discussion that in very many cases the tumors could be removed by the vagina. Before the question of the Battey-Tait operation should even be considered, the possibility of their removal by the vagina should be absolutely excluded. If the case is such that from the situation of the tumor, or, from its size, its vaginal removal is plainly impossible, it by no means follows that the other is in order. The statement is often enough made that the removal of the uterine appendages will arrest the growth of these tumors,—a number of cures have been reported,—but I think on insufficient evidence. There are several facts that stand much in the way of accepting this belief, and one of them is that many of these neoplasms have their most luxuriant growth after the menopause. Such is notoriously the case with cancer of the uterus; and is it not a fact, settled beyond dispute, that it first begins to show itself coincidently with a cessation or diminution of ovarian activity? I was very much struck, in reading recently the report of Dr. Thomas Keith's cases in the Edinburgh Royal Infirmary (reported by his son, Skene Keith), with the remarkable series of ten successful hysterectomies. In two of these cases the uterine tumor first gave evidence of its presence at or after the menopause, while the average age of all ten patients was thirty-six years. Similarly, Dr. Drysdale has recorded it as his experience that these affections were apt to develop more rapidly after the menopause (Am. Gyn. Soc., 1880).

Furthermore, it has been stated by competent observers that fibroids of the uterus have been known to disappear during pregnancy. The growth has been not only arrested, but the already formed tumor has undergone absorption. In other words, the highest functional activity of the organ sometimes has brought about more than what is claimed for the exactly opposite procedure of abolishing all functional activity whatever. That some of the reported cures are premature, the case of Trenholme already cited abundantly proves. Even if it were proven that the Battey-Tait operation would produce the result claimed, we are met by the practical difficulty that in many cases which peculiarly appeal for help,—those with very large tumors weighing twenty, thirty, and even forty pounds,—it is impossible to remove the uterine appendages without doing hysterectomy.

The ovaries are often imbedded so firmly in the fibrous mass, that even the most careful dissection would not enable us to remove them with safety to the patient. The larger the tumor and the more need there is for relief, the less likely is the Battey-Tait operation to be either practical or successful. And even in the cases of smaller tumors, where their vaginal extraction is out of the question, we must conclude that the curative effect of the

Battey-Tait operation is "not proven." If it be asked how I account for the reported cases of such cures—or, rather, arrest of growth and subsequent shrinkage—I think many have to be thrown out like Trenholme's, because reported too soon. In others, the temporary modifying influence of such a radical invasion as this makes on the nutritive processes of the general system accounts for much. Until it is proved that fibrous and other tumors of the uterus arise from ovarian influences, I believe it is sound teaching to deny the justifiability of the operation for their arrest of growth. I think that this whole class of cases, those of uterine hemorrhage and uterine neoplasms, are not suitable for the Battey-Tait operation. The operation has been advanced on unsound physiological principles, and is, therefore, to be condemned as bad practice.

V. The last division of our classification embraces cases of diseases of the uterus and Fallopian tubes, for which it has been proposed to do the Battey-Tait operation.

As regards the chronic diseases of the uterus like endometritis of long standing for which this suggestion has been made, I do not know of any cases in which it has been carried into execution. The reasons already advanced for doubting the efficacy of the proceeding when the uterine symptoms are hemorrhagic or nervous, hold to an increased degree here. The effect that the subsidence of ovarian influence would have in modifying uterine malnutrition is so largely unknown, that no one would be justified in taking such a perilous leap in the dark. Until, therefore, much more definite information is gained, more especially by experimental pathology, on this point there is hardly any use in discussing this point. But a much more important set of cases are those of hydro-salpinx and pyo-salpinx, which Mr. Tait was the first to operate on. Here the conditions are quite different, for the diseased organs themselves are the seat of the proposed operations. They are inaccessible to direct local medication, and from their relation to the peritoneal cavity, are liable to be the foci from which much graver mischief may be set up. We owe very much to Mr. Tait for his great additions to our knowledge on this subject, for before his writings appeared the whole region of the tubes in disease was practically an unknown country. It is remarkable that we have remained in ignorance on these points so long, for years ago Henle's description of the anatomy of the tubes might have suggested the extreme liability of the dilated part (the ampulla) becoming the seat of collections of fluid. And further, the occurrence of tubal pregnancy with its terrible mortality, should have drawn attention to the other possibilities of disease they possessed. Mr. Tait has done especially valuable work in calling the attention he has to the importance of gonorrhœa in producing salpingitis. His researches receive remarkable confirmation from the views expressed by Dr. Noeggerath on latent gonorrhœa in the female. The late Dr. Bumstead used to teach that gonorrhœa was more fatal to the human race than syphilis, and I certainly believe that is true in the female half of it. Gonorrhœa in the woman is apt to be a more serious disease than in the male, because of the different terminations of the Fallopian tube and the vas deferens. Once given an attack of gonorrhœa extending into the tubes on both sides, and the chances are overwhelming that the patient will be rendered sterile, and probably, if unrelieved, a confirmed invalid. Even though the inflammatory process stops in the tubes, and does not travel on, but gradually subsides, the products of inflammation are such as to render the the tubes useless as oviducts.

When the inflammation cannot be traced to gonorrheal infection, and even if that etiological factor can be excluded, if pyosalpinx has once occurred, I think the above holds good. In opening up this field for the operation, Mr. Tait has done a great service, and his brilliant series of over sixty consecutive successful operations recently reported at the London Obstetrical Society meeting, shows how wide it is. When we come to his similar treatment for simple hydro-salpinx—dropsy of the Fallopian tubes —I am not inclined to agree with him. Tapping seems to me destined to play a very important part here, both in establishing the diagnosis, and also in curing the patient. The microscope may be made to subserve a most useful purpose in determining the differential diagnosis between hydro- and pyo-salpinx, and consequently, I take it, in the justifiability of the operation. It seems possible to settle two things by tapping; first, what is the nature of the fluid? Are we dealing with a collection of pus, the consequence of an inflammation, specific or not, or with an effusion of serum? Second, does the fluid come from the Fallopian tube? In determining this second point my friend Dr. Frank Ferguson of New York has made a most admirable suggestion. As the result of a large number of examinations, he concludes that the presence of cells of columnar ciliated epithelium, is pathognomonic of salpinx. This test seems so simple and so rational that it should be resorted to in all suspected cases, and I hope its author will get the credit he deserves for advancing so beautiful and logical a diagnostic sign. In a given case, therefore, should we determine from the fluid withdrawn that it was from the Fallopian tubes, the further question of operation would depend on the presence or absence of pus in the fluid. If the case be one of hydro-salphinx the tapping is likely to prove curative and the operation is uncalled for. If it should prove to be pyo-salpinx aspiration is not likely to suffice, and after waiting for a short time to see, it would be right to resort to the Battey-Tait operation to effect a cure.

Conclusions.—Looking back over our analysis of the indications for which the Battey-Tait operation has been resorted to, I believe that the following deductions will hold good:

I. The operation is not justifiable for purely nervous symptoms independent of clearly marked evidences of organic ovarian disease.

II. It is not justifiable in cases of uterine disease or hydro-salpinx.

III. It is not justifiable—in the light of our present knowledge,—for the relief of uterine hemorrhage, or the arrest of the growth of uterine tumors.

IV. It is justifiable as a last resource—all other rational methods of treatment having failed: (*a*) In certain malformations and injuries of the utero-vaginal canal, rendering menstruation impossible ; (*b*) In incurable organic disease of the ovaries ; (*c*) In pyo-salpinx.

The cure which the Battey-Tait operation offers to the large number of sufferers who constitute the fourth class, is one of the most glorious achievements of the surgery of our age.

William M. Thallon, M.D.

NEW BOOKS AND INSTRUMENTS.

The International Encyclopædia of Surgery. A systematic treatise, etc. Edited by John Ashhurst, Jr., M.D., in six volumes. Vol. iv. New York: William Wood & Company, 1884.

The fourth volume of the work begins with a very meritorious article on *Injuries of Bones*, by Dr. John H. Packard, of Philadelphia. The pages bear throughout evidence of the author's thorough practical familiarity with the subject, and it is very agreeable to read, not only a compilation of what others have written, but also what Dr. Packard thinks on the several important details of his matter. As regards the theoretical part of some mooted questions, such as, for instance, Colles' fracture and fracture of the neck of the thigh, we find in Dr. Packard a commendable writer, inasmuch as he shuns all semblance of pedantry and learned buncomb. It would be difficult to name a modern author on fractures who, by attempts to be exhaustive and learned, does not occasionally become a bore. Long-winded disquisitions and quotations, instead of quickening the understanding, only muddle and exhaust the reader's intellect, and a subject like "fractures" must become "dry as dust," unless a clear-headed, practical writer knows how to infuse into it some of his own warmth and life.

There are, of course, a certain number of views on which issue might be joined with Dr. Packard; but who would quarrel about the "so-called antiseptic treatment" with an author who succeeds in both interesting and instructing his readers.

Dr. Packard justly condemns the "immediate" bandage, warmly recommends early passive motions, and in speaking of the treatment of union with deformity, strangely omits to mention McEwen's osteotomy with its brilliant results.

Among the improved means for treating fractures of the ribs, the application of the elastic bandage is not mentioned. It is far superior to the irritating and unyielding adhesive strips, or even to a flannel bandage cut "bias."

The best parts of the article are those referring to the fractures of the clavicle, the radius, and the thigh.

Richard Barwell, of London, furnished the article on *Diseases of the Joints*. The difficult matter is treated with a master hand, and belongs to the best of the collection. Commendable is the systematic arrangement of the several parts under three headings: morbid anatomy, symptoms, and therapy. The attention paid to the pathology of the joints by the skilful writer, will captivate the reader's attention all the more, as the vivid, almost plastic description of trite, unattractive themes, shows the writer to be a man of varied knowledge and culture.

Mr. Barwell's nomenclature of synovitis seems to be unfortunate, because ambiguous. *Purulent* and *suppurative* synovitis is to most minds an identical thing; yet Mr. Barwell names "*purulent*" a variety of simple synovitis, whereas the acute phlegmonous inflammation of the joints is called by him "*suppurative*."

Evacuation of the joint in simple synovitis is justly recommended as a safe measure. In the preceding paper, Dr. Packard condemns it as dangerous and unjustifiable. But the *principle* of antiseptics is insisted on as a *conditio sine qua non*. Barwell has grave doubts as to Dr. Martin's assertion, that evacuation with subsequent elastic compression will always cure synovitis, even when the patient is directed to use his limb. The appropriate use of shampooing, massage, and gymnastics is recommended, not without a sarcastic side-glance at the pretensions and exaggerations of Mosengeil.

Suppurative and "urethral" synovitis form excellent chapters, full of valuable lessons based on personal experience.

In the treatment of synovitis by absorption, parenchymatous, or, as Barwell calls them, "interstitial" injections of different antiseptic agents with a tubular needle, are recommended. Hueter's similar, if not identical, efforts, made as early as 1873, are not mentioned.

The chapter on *Strumous Synovitis* shows evidence of excellent observation and careful study of phenomena, wide literary knowledge, and no mean skill in presenting this unattractive subject to the reader. Dr. Schüller's experimental studies regarding tuberculosis of joints provoke sharp criticism on the part of the author, who, however, is unable to furnish a better substitute for the microzymic theory.

Mr. Barwell gracefully acknowledges the priority of American surgeons in conceiving and successfully applying the principle of portable extension apparatus in joint affections. The objection to Sayre's adhesive-plaster contrivances seems to be well taken: skin irritation, loosening, and the necessity for frequent renewal being their undeniable accompaniments. The naïve Englishman does not see the mercantile value of just such defects.

Rheumatic, gouty, syphilitic, and neurotic joint affections all receive due attention, hip-disease having the distinction of a separate chapter. Among the many forms of hip-splint, permitting locomotion of the patient, Dumbrowsky's is declared to be the best.

The part on the treatment of anchylosed joints contains all that is worth knowing about the subject. Can it be accidental that McEwen's name does not occur at all among those recommending osteotomy? What is known as McEwen's method, however, is minutely described by the author. A "misunderstanding" about priority seems to be lurking at the bottom of this matter.

A short but pregnant article by Dr. John Ashhurst, Jr., treats of *Excisions and Resections*. The value of the article is much impaired, however, by the author's anachronous attitude of negation respecting the value of the antiseptic wound-treatment. Dr. Ashhurst little thought, when quoting the words of the younger Moreau, how well they might be applied to himself. The elder Moreau having successfully practised excision of a diseased joint, "laid his views, based upon this experience, before the Academy of Surgery, definitely proposing the operation of complete excision of diseased articulations. In 1786 the same surgeon communicated to the Academy the history of a case, in which he had in that year successfully removed the whole shoulder-joint for disease, but neither of these communications seems to have excited interest. A third memoir, presented in 1789, met with almost universal disapproval and condemnation, the learned members of the Academy *finding it more convenient to deny, than to examine, the facts on which it was grounded, etc.*"

It is said on page 458 that chloroform was preferred by surgeons to ether in excisions of the upper jaw on account of its vapor being non-inflammable by contact with the hot iron. But finding that there is no risk of setting the patient on fire, Dr. Ashhurst has of late given the preference to ether.

It seems, accordingly, that the author does not appreciate the additional danger of pneumonia, caused by the use of ether in operations done about the respiratory tract. Enormous masses of mucus are secreted, and becoming mixed with blood are aspirated into the bronchi to cause irritation.

Billroth's remarkable results in some fifty cases of exsection of the maxillæ treated with iodoform dressings, are not mentioned. König's celebrated case of total excision of the sternum for sarcoma is presented incorrectly ; there was no suppuration of the pericardium.

Langenbeck's method of resection of the wrist by one dorsal incision is omitted, although many surgeons regard it as far superior to Lister's procedure.

The author recommends in excisions of the hip-joint always to remove the trochanter. He is, on the whole, a warm advocate of excisions where properly indicated, and furnishes a number of histories of his own cases.

Dr. George E. Fenwick, of Montreal, winds up the subject with a short paper on *Excision of the Knee-Joint*, in which his peculiar manner of sawing the bones with a fine fretwork saw is described. Twenty-eight, mostly very successful, cases serve as a basis.

Tumors are disposed of by Mr. Henry Trentham Butlin, of London. The article is unpretentious, but that is its greatest merit.

Among the several views held about the etiology of tumors, the author inclines to the parasitic theory, because it "offers at the present moment greater *attraction* than any other" ; likewise does Mr. Butlin favor Gussenbauer's unconfirmed theory of the spread of tumor-infection to adjacent lymphatic glands by minute molecules, because the "statements"—contained in Gussenbauer's paper—"are made *in so confident a tone that his theory is very attractive.*"[1] And this by a countryman of Bacon !

Waldeyer's theory is accepted as the most convenient, and the classification based upon it. The different forms of tumor are then discussed *seriatim.*

As a compact elementary treatise of tumors, the article will undoubtedly fulfil very well the wants of the general practitioner.

A number of well-executed lithographic plates, some of them colored, accompany the article.

The late John A. Lidell's monograph on *Injuries of the Back*, will not disappoint those in search of authentic information on this most difficult, and hence, it seems, most neglected, branch of surgery. The wealth of illustration, in the shape of a carefully selected casuistry of his own and of the experience of other surgeons, is knit together by the terse and clear text into a compact whole, fully up to the best work of the author.

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[1] Reviewer's italics.

Part I. treats of the injuries of the soft parts ; part II. of those of the vertebral column, the latter forming one of the best chapters of the essay. Dislocations, fractures, and gunshot injuries are considered in succession. As regards the last-named injuries, exception must be taken to the advice given by Lidell, that, "if the missile has lodged, it should be found and extracted, *if possible.*" This means that an attempt should be made any way,—a doctrine the soundness of which is gravely questioned by most surgeons of the present day.

The injuries and traumatic inflammation of the spinal cord and spinal nerves, together with the neuro-trophic changes arising therefrom—as, for instance, bed-sores, etc., are disposed of in part III. The article winds up with a consideration of the injuries of the sacrum and coccyx, and with a chapter on the remote effects of spinal injuries, as railway spine and others.

Malformations and Diseases of the Spine, by Mr. Frederick Treves, of London, form the subject of the last article of the volume.

Of the several modes of treating spina bifida, Morton's method of injecting the sac is recommended as the safest.

"*Cyphosis*" and "*lordosis*" are used to denote real *curvatures*, not angular deformities of the spine. *Spondylitis deformans* is the term used by Mr. Treves to denote chronic rheumatic arthritis of the vertebral column ; it is said that *its use is justified by custom.*

It is gratifying to read the author's contemptuous remarks about the absurd term "angular curvature," the blundering invention of an illiterate doctor.

The retention of the unmeaning term "Pott's disease" is earnestly urged, one of the most curious arguments for this being, that "the term commits the user to no particular pathological opinion."

The author shows good sense in disposing of the silly dispute about the constitutional or traumatic origin of Pott's disease with a very few words.

The pathology of the disorder receives very thorough attention, the symptomatology being mainly superimposed on the safe basis of its morbid anatomy. Recumbency is declared to be the most important form of affording relief, and the host of mechanical appliances, including the different kinds of "jackets" and "plastrons," are subjected to a very wholesome, though not unfair, criticism. Indiscriminate suspension is justly declared to be dangerous; likewise is the wearing of the same plaster jacket for more than three months condemned.

On the whole, it can be said that Mr. Treves has produced a very creditable piece of work. [A. G. G.]

Practical Histology: A Manual for Students and Practitioners. By G. Sims Woodhead, M.D., F.R.C.P.E., etc., with one hundred and thirty-six colored plates. American edition. Henry C. Lea's Son & Co., Philadelphia, 1884, 8vo, pp. 484.

The appearance of an American edition of this manual is to be hailed with pleasure, as giving in a clear, compact style the elements of pathological histology in such a manner as to be useful alike at the laboratory table and as a work of reference. But we believe that its chief excellence will prove to be as the constant companion of those working pathologists who look beyond mere naked-eye appearances in their study of morbid anatomy.

The first two chapters of the work give a brief but sufficient account of the instruments and reagents required for pathological examination, and explicit though incomplete directions are given for the opening and study of the cadaver. In the second chapter microscopical *technique* is detailed in a spirit of critical selection, and in view of immediate utility. The newer processes of staining, more especially as applied to bacteria, are given. We are pleased to see that Dr. Woodhead recognizes the merit of picro-carmine in these words: "By far the most useful reagent at present in the hands of the histologist is Ranvier's picro-carmine staining fluid"; and that he condemns the still too common practice of cutting sections by hand as a waste of time. The freezing microtome, with ether or salt, is now made so cheaply and yields such uniformly good results that its use should be taught all students. We have known students and even medical men greatly discouraged in their histological work, and almost tempted to give it up, because they could not acquire sufficient skill (and very few can) in cutting by hand. Even for hardened specimens, the microtome with an embedding mass is preferable. The time thus saved can be devoted to the much more important manipulations of staining and to *study* of the specimens.

The favorable mention of Valentin's knife (page 33) we can also endorse from personal experience, at least as regards the more consistent organs, such as the liver and kidney.

The normal and pathological histology of the various tissues, organs, and apparatuses is presented in nine chapters. In these chapters appear the almost inevitable (in view of the small size of the book) inequalities and omissions of the work. Still a better proportion, with an eye to practical requirements, might have been preserved between the various chapters. For instance, the pathology of the lung and of the kidney is quite fully presented in one hundred and thirty pages; the spleen receives an undue share of attention in twenty-five pages. On the other hand, the pathology of the brain is disposed of in five (!) pages, and that of the entire nervous system in twenty-two. Nothing is said of such important lesions as encephalitis, myelitis, pachymeningitis, sclerosis, etc. Lesions of peripheral nerves, of muscles, of the skin, are not treated. That the pathology of the special nerves should be altogether omitted is more pardonable, considering the scope of the manual.

Tumors are well analyzed and illustrated in a chapter of sixty-four pages. The work closes with two chapters on parasites, animal and vegetable; and in the last, bacteria receive suitable attention.

The illustrations, nearly all chromatic, are very good—many of them admirable. These, with the terse practical directions for the preparation of specimens, must constitute the strong feature of the manual. It is well printed, and, a strong point for a work-table book, it is so bound as to open and lie open easily and fully. [E. C. S.]

Transactions of the American Gynecological Society. 1883. Vol 8, pp. 276. D. Appleton & Co., New York.

In reviewing the Transactions of this Society for 1882, in the last number of the ARCHIVES, we drew attention to their extreme tardiness of apppearance, and it is therefore with the greater pleasure we note the improvement made in the prompt publication of the present volume. The book before us maintains the high excellence of the series in the matter of paper and type, although the publishers are changed. The proof-reading shows, perhaps, a little less care, notably in the report of the discussion on pp. 247 and 249, where several palpable errors have crept in. The excellent index of current Gynecological Literature, which has been a feature in former volumes, is omitted this year, and the number of pages is considerably less than usual, owing to the average length of the papers read being less.

The Eighth Annual Meeting of the Society was held in Philadelphia on Sept. 18, 19, and 20, 1883, and fourteen scientific papers were presented :

I. The Annual Address by the President, Dr. Gilman Kimball, of Lowell, Mass, was devoted to a "Biographical Sketch of Dr. Nathan Smith, Founder of the Dartmouth Medical College." The President traced in a most interesting way the main features in Dr. Smith's life, laying particular stress on the enormous amount of work accomplished by him. The author emphasized in particular how much the cause of medical education, more especially in the Eastern States, was indebted to Dr. Smith's heroic labors. He went on to portray his hero's eminent qualities as a surgeon, and wound up with an account of the famous case of ovariotomy, justly claiming that though subsequent to McDowell's, it was done without any knowledge of it. He therefore claimed for Dr. Nathan Smith a like degree of honor to that paid to the memory of the "Father of Ovariotomy."

II. "Superinvolution of the Uterus," by Dr. Joseph Taber Johnson, is an interesting contribution to the study of this disease, first described by Sir James Simpson thirty years ago. He narrates four cases that have come under his own observation—in none of whom had treatment proved of any value. In the discussion, Dr. Fordyce Barker made an important contribution to the subject, by pointing out that cases of superinvolution of the uterus should be divided into two classes—one in which there was accompanying superinvolution of the ovaries, the other in which these glands retained their functional activity. In the first class his experience coincided with Dr. Johnson's, that all treatment was useless ; but in the second, treatment, especially galvanization of the uterus, had proved of great value in two cases he narrated, in both of whom pregnancy subsequently occurred.

III. "Cleanliness in Surgery," by Dr. R. Stansbury Sutton, is a fair statement of the Doctor's observations in Europe, more especially with reference to abdominal surgery. The general consensus of opinion, both abroad and at home, to judge from the discussion, is in favor of the abandonment of the Listerian spray in this department.

IV. "Hot Water in Secondary Hemorrhage after Pelvic Operations," by Dr. Albert H. Smith, draws attention to a hæmostatic, the value of which he deems insufficiently appreciated. He narrates an interesting case in proof of his position, but neither in his paper nor in the discussion is an explanation of its mode of action offered. Dr. Barker raised the point that a certain time—sometimes even fifteen or twenty minutes—must elapse before you get the hæmostatic effect, and that therefore in some cases—he instanced one—it might not be of avail because of the delay. It seems to us before we can unreservedly accept Dr. Smith's conclusions, it is necessary to determine whether the hæmostatic effect is due to direct coagulation of the tissues—as in the use of the cautery—or to a reflex action producing contraction of the muscular elements of the vessels. We believe that the temperature of the water (115 ° to 120 ° F.) that the Doctor gives, necessitates our adopting the second hypothesis, and if so, though often of great value, it will fail at times, like all other hæmostatics which act through the same mechanism, from defective innervation. We are therefore inclined to believe that this method is more applicable in secondary hemorrhage, than during an operation when the patient is profoundly anæsthetized, and all reflex action is wellnigh abolished.

V. "Some Points Connected with the Subject of Dysmenorrhœa," by Dr. C. D. Palmer, lays especial stress on the neurotic factor in the disease. He shows the insufficiency of the doctrine of its causation being due to mechanical obstruction, such as stenosis or flexion, as taught by Simpson and Marion Sims. While not denying the validity of this explanation in some cases, he instances others in which obstruction exists without dysmenorrhœa, and dysmenorrhœa without obstruction. He believes the vast majority of cases to be neurotic in their pathology, and he relies accordingly far more on the value of general than on local treatment. The gentlemen who participated in the discussion endorsed, in the main, the conclusions of the paper, and their general adoption would lead to a gratifying diminution in the number of incisions of the cervix.

VI. "A Rare Form of Abdominal Tumor: Three Cases, with One Specimen," by Dr. T. A. Reamy, describes three interesting cases of cystic tumors unconnected with the ovaries and uterus. Two were relieved by tapping; the third being shown by the post-mortem to take its origin from the omentum, numerous sarcomatous tumors having developed in the cyst wall. If the author's diagnosis is correct, that his two cases were not malignant, but would probably have become so if unrelieved—like the third,—

the question of resorting to the operation for their removal in the first place is of importance. But until more cases are put on record, his plan of tapping will probably be the safer practice.

VII. "On a New Mode of Operating for Fistula in Ano," by Dr. Edward W. Jenks, narrates two cases in which he incised the fistulous tracts in the usual manner, and after dissecting out the so-called pyogenic membrane or lardaceous and cartilaginous tissues along the fistula, he united the freshened surfaces. Externally the sutures were introduced as in operating for lacerated perineum extending through the sphincter; while in the gut, where it was necessary to bring the divided surfaces in apposition, he used interrupted silk sutures. In this way union throughout was secured by first intention in ten days, instead of having to await the ordinary slow process of healing by granulation. In addition he claims that the risk of incontinence is greatly lessened. The operation is an eminently rational one, but in introducing it as something new the author is in error, for by referring to the *Chicago Medical Review*, some four or five years back, he will find this same plan described by Dr. E. P. Dudley; in a subsequent article on the same subject, the suggestion is endorsed by Dr. Alex. J. C. Skene.

VIII. "Congenital Fissure of the Urethra with Exstrophy of the Bladder," by Dr. Henry F. Campbell, gives the history of a case of a little girl four years old, in whom this error of development is partially present; the most interesting point in connection with this case being as to the proper time for operating to repair the deformity. The author intends to wait until after puberty before attempting to close the fissure, in order to gain the advantage of the patient's intelligent co-operation, and the increased room in the pelvis for his manipulations, and the more abundant tissues to bridge over the cleft.

Dr. Mann in discussing the paper urged (justly, we think) an early operation, on the ground that there was more space and tissue than is generally supposed, and because the parts would then develop more naturally.

IX. "The Management of Accidental Puncture and other Injuries of the Gravid Uterus as Complications of Laparotomy," by Dr. Charles Carroll Lee. The author has collected the literature of these rare accidents, comprising six cases, to which he adds one of his own. As a result of a careful study of the question, he comes to the conclusions that if the injury to the uterus also involves its contents, then Cæsarean section should be at once performed ; if, however, the muscular wall simply be injured, as in his own case, it will suffice to carefully close the wound with carbolized sutures.

X. "Is the Extirpation of the Cancerous Uterus a Justifiable Operation ?" by Dr. A. Reeves Jackson. This paper contains an emphatic negative to this question, and the author bases his conclusion on a careful study of the recorded cases. He shows that more than seventy-two per cent. of the cases operated on, by the abdominal method, die at once ; while the remaining twenty-eight per cent. die inside of a year. The operation of complete removal by the vagina is not much better in its results. The author therefore justly concludes that the entire disease is never completely removed, and that the aggregate amount of suffering is not lessened by the operation, while the average length of life is curtailed. In the discussion the various participants narrated their personal methods of operating in these cases, but as they were methods of partial removal by the vagina, they did not conflict with the conclusions of the paper.

The only exception to this was Dr. Sutton, who stated he had done the complete operation five times, but he did not give his results.

XI. "A Study of the Etiology of Perineal Laceration, with a New Method for its Proper Repair," by Dr. Thomas Addis Emmet. The distinguished author begins by stating that all the ills usually supposed to follow a laceration of the perineum are really due to a transverse laceration, by which the parts lose the support afforded by the pelvic fascia. In order, therefore, to afford the patient true relief, some portion of the posterior wall of the vagina must be included in the line of union. It is not necessary to denude as much tissue anteriorly as is done in the ordinary operation of perineorrhaphy, as taught in Dr. Emmet's book. The author, in this new operation, denudes two crescentic lines of tissue : The upper one is given by three points,—the lower portion of the carunculæ on each side and a corresponding point on the posterior vaginal wall,—these three points being drawn together by tenacula first, and so determined ; the lower crescent is then formed in the ordinary way, but is less extensive than in the old operation. In the discussion Dr. Reamy wittily remarked that if this new operation ought to be substituted for the former one which Dr. Emmet has so admirably described in his work, it is fortunate that he has the opportunity to undo the great evil which he has done in teaching us a false or useless operation. It is difficult to criticise a plastic operation without seeing it done and examining the result, but it seems reasonable that Dr. Emmet has added an improvement by his new plan in cases of extreme rectocele. At the same time we cannot see why in the ordinary operation his object is not attained far more simply by a single crown suture, which is passed through the upper angles of the denuded tissues and includes the lowest point in the posterior wall in its grasp. The same fault which characterized Dr. Emmet's contribution last year "On a New Method of Treatment for Diseases of the Urethra," is evident in this paper—the sweeping condemnation of all other methods, except the new one about to be brought forward. It is, however, consoling to reflect that in this instance the teacher whom the Dr. Emmet of to-day will first have to refute before his new dispensation can be accepted, is the Dr. Emmet of twenty years ago.

XII. "Remarks on Chronic Abscess of the Pelvis," by Dr. William H. Byford. This is one of the most interesting and original papers in the present volume. The author traces the changes that take place, more especially in cellulitis and hæmatocele after resolution has occurred. He traces the natural course of the abscess if unrelieved—the routes it will take to discharge, and the chances of subsequent cicatrization occurring. He finds that this depends mainly on the conditions as to compression of the surrounding tissues and the state of the abscess wall. He shows that suppuration is kept up by the granulations lining the wall of the abscess, and if these are thoroughly removed and the walls kept in proper opposition, union by cicatrization occurs. He therefore advocates the practice of free drainage per vaginam in most of these cases, together with the removal of the tags of granulation tissue projecting into the abscess cavity by means of a curette.

The paper is a model of accurate and minute observations and logical thinking. In the interesting discussion which followed, Dr. Byford's views were strongly endorsed, the additional point being made by Dr. Thomas: he believed in following Mr. Tait's plan of abdominal drainage in those cases where the safer method of Dr. Byford was inapplicable.

XIII. "Ergot: the Use and Abuse of the Dangerous Drug," by Dr. George J. Engelmann. The author preaches a sermon from the text that "ergot should only be given in the non-gravid uterus." This so exactly corresponds with the views expressed by another member of the Society (Dr. Johnson), in a paper read at the previous meeting, that it would have been better had the author at least alluded to the coincidence. The style of this paper cannot be commended, for it is quite unnecessary, in protesting against the evils resulting from prolonged tonic spasm, to do so in a manner strongly suggestive of the clonic variety.

XIV. "Rectal Disease Considered in its Relations to Disorders of the Uterus," by Dr. R. B. Maury, is the paper presented by the new Fellow of the Society. He shows first the essential unity of the vascular supply of the bladder, uterus, and rectum; how they are all supplied, almost exclusively, from the same arterial source; how the venus plexuses all terminate in one channel, the internal iliac vein; and how closely connected they are by this nervous supply. From these facts he deduces the close relations of the parts in disease, in hyperæmia, in chronic inflammation, and chronic ulcer of the rectum. Perhaps the most interesting point in his excellent paper is the explanation he offers for the greater frequency of occurrence of rectal ulcer in women than men.

A memoir of the late Dr. James Dowling Trask, of Astoria,—a beautiful tribute to the memory of a good man,—by Dr. Fordyce Barker, concludes the volume.

This volume as a whole is worthy of the Society from which it emanates. The papers are mainly clinical, and consequently of great practical value; the discussions which they provoked are generally to the point, and give the ripe experience of men—the best qualified to teach us on these subjects. [W. M. T.]

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ARCHIVES OF MEDICINE.

Original Articles.

TRACHOMA AND THE ETIOLOGY OF JEQUIRITY OPHTHALMIA.*

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THE motive for presenting to this society a paper on a disease so common as trachoma, is furnished by two circumstances, *i. e.*, the recent introduction by Dr. de Wecker of Paris of a new therapeutic agent for the treatment of this rebellious affection, and a new view of its etiology advanced by Prof. Sattler of Erlangen.

If we may measure the importance of a disease by the frequency with which it is met in practice and by the amount of controversy to which it has given rise, then *trachoma* or *granular conjunctivitis* deserves a conspicuous place in medical literature.

Permit me to say, at the outset, that whatever criticisms I may make regarding the histology of the conjunctiva and the tissue changes in granular conjunctivitis, are based upon examinations of eighteen healthy human conjunctivas at ages varying from the time of birth to the thirty-first year, and forty-seven cases of trachoma examined at different periods of the disease during the past six years. The chapter on the *etiology of jequirity* is the outcome of experiments made on a large number of rabbits and dogs; and that part of the paper which deals with the *bacterial view of trachoma,* is based upon examination of thirty-seven cases of trachoma with this end in view.

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* Paper read before the New York County Medical Society, April 28, 1884.

ETIOLOGY.

Granular conjunctivitis is developed most frequently by contagion,—by the transportation of secretion from the eye of a patient with the disease to a healthy eye. Yet, we know that blennorrhoic pus may give rise to granular conjunctivitis in another, although it may never develop in the original eye; and cases of this disease certainly do occur in persons who have never been brought in contact with a patient who is the subject of granular conjunctivitis. Von Græfe maintained that the contagion was transmitted through the atmosphere. This much we do know, that the disease in question does sometimes occur epidemically among individuals living under the same bad hygienic conditions. But just why blennorrhœa will develop in one case into trachoma and not in others is not explained, in fact, by the supposition that the one disease is *bacterial* in its origin and the other not. *The numerous examinations and cultures of blennorrhoic pus have convinced me of the pretty constant presence of a micrococcus, but that there is no morphological or physiological difference between it and the micrococcus found in trachoma pus;* and we are forced to speculate that trachoma is a simple blennorrhœa produced by bad hygiene, which so modifies the organism that the disease assumes a particular condition. To what degree we should regard *diathesis* or congenital predisposition responsible for trachoma is a question of great interest and importance. If a particular bacterium were responsible for the disease,—this bacterium, like that of tuberculosis and typhoid fever, etc., inhabiting the air,—we could still reconcile ourselves to the immunity or rather escape of many individuals from trachoma. However, we have, in the idea of the bacterial origin of trachoma, a hypothesis which experimentation alone must prove or disprove.[1] Prof. Sattler, who first announced the discovery of a micrococcus in the trachoma tissue, claims to have succeeded in reproducing the disease with the elements derived from the culture of the parasite, which he considers to be the generator and vehicle of the contagion.

My own investigations are diametrically opposed to Sattler's view. I have succeeded in cultivating in sterilized gelatine bouillon, and blood-serum culture media, a micrococcus variety derived from the trachoma secretion or pus, but I have never been able to reproduce with pure cultures (for we often find many different forms of bacteria in these secretions) a disease resembling trachoma. I have made diligent examinations of over 1,500 sections derived from thirty-seven cases of trachoma—the piece of tissue for examination having been excised during life, and placed at once in strong alcohol, and subsequently embedded in celloidin, and the thinnest sections made with a microtome, and subjected in many cases for a longer time than usual to the action of one of the aniline coloring agents, methyl blue, methyl violet, fuchsine, Vandyke brown, Hoffman's purple, and decolorized with alcohol, or acetic acid, and mounted in glycerine or balsam without chloroform, an Abbe's condenser being used in every instance.

Sattler [2] acknowledges that micrococci are absent in some cases from the secretion in trachoma at certain periods of its course, when, he says, the secretion is not contagious. He states that from seven to eight days after inoculation of the selected culture medium (blood serum) with secretion from trachoma, there spring up at the site of inoculation a few pale, yellowish points; and that he found exactly the same micrococcus in the trachoma tissue as in the secretion. He speaks of the micrococci clinging to the exterior of the nuclei.[1] If these micrococci actually do exist in the trachoma tissue I have never been able to find them. It is so very easy to be mistaken as to the identity of certain bacteria, especially those of the micrococcus tribe. We know how closely micrococci are simulated by nuclear detritus in the blood, by the "Mastzellen" of Ehrlich—a large granular cell found in inflamed tissues—and although these objects may usually be distinguished from micrococci by their rarely being of uniform size as micrococci are, and by their reaction to certain staining agents, yet the positive proof, after all, is usually only furnished by cultivation outside of the body. In examining this question for myself, I proceeded as follows:

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[1] Bericht über die Versammlung der Ophthal. Gesellschaft, Heidelberg, 1882.

[2] *L. c.*, p. 173.

After thoroughly washing the conjunctiva of a given case of trachoma with a solution of bichloride of mercury, too weak to destroy micro-organisms after so short an application, this being used only for its mechanical effect to remove all secretion, and because it would not be likely to be itself contaminated with micro-organisms as in case plain water were used. The scissors and forceps were sterilized by heat, and one or more trachoma granules were excised and placed in the sterilized gelatine bouillon, and transferred to the incubator at a temperature of 35° to 36° C. In some instances, despite my precautions, the culture medium was contaminated by a variety of bacteria, but inoculation of rabbits' and dogs' eyes with these failed to produce trachoma. But in a large number of instances the culture medium remained free from bacteria for several weeks, until they were thrown away. However, I am still engaged upon this subject, because I believe it requires further investigation.

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[1] "Bericht der Ophthal. Gesellschaft," Heidelberg, 1881, p. 24.

Experiences must be multiplied, and if they conform to one another, then only can we have a true conviction. This fact remains: inoculation with trachomatous pus produces in some cases an acute inflammation, which may disappear without leaving a trace of mischief, or it may give rise to granular conjunctivitis. If there were a specific element you might say it should always reproduce the same disease and not another. But even such an argument would not dispose of the idea of the specific character of the disease in question. As already stated, I have found micrococci in blennorrhoic and trachomatous pus (if I may take the liberty with the term), and there is nothing unusual in this circumstance, but although apparently identical morphologically, I have not succeeded in establishing by cultivation a physiological difference—*i. e.*, in isolating a micrococcus or any other variety of micro-organisms which is capable of producing trachoma; nevertheless, it may exist. And even should Sattler's views be disproved by others, they do not deserve to be ridiculed with criticism based upon hypothetical reasoning. His analyses are evidently honestly meant, and have been fairly conducted, and should be treated in a more liberal spirit than has been accorded to similar views of other writers in connection with other diseases.

It is a curious circumstance that purulent conjunctivitis, and especially blennorrhœa, may give rise to trachoma; and in view of recent developments, or rather contradictions, it would be less hypothetical to maintain the existence of a close relationship between granular conjunctivitis and other forms of conjunctival inflammations; and when we find something special, as in trachoma, to refer this to the individual and not to the disease itself. But in what degree a certain diathesis is responsible for this disease, it is difficult, even in the presence of statistics, to speak with positiveness. At any rate it is a significant circumstance that the granulations attack by preference individuals the state of whose lymphatic system predisposes them to the production of certain diseased conditions. Trachoma is comparatively rare among those whose means permit them to live under good hygienic conditions, and in healthful climates; and these conditions generally influence trachoma favorably. This, of course, is characteristic of every disease which is due to a mischievous diathesis or deterioration of the organism from whatever cause. The same may perhaps be said respecting the *influence of race.*

Dr. Swan M. Burnett,[1] of Washington, D. C., has directed attention to the immunity of the negro from trachoma. In a letter which I have recently received from this gentleman, he stated that he intended to publish shortly the results of his investigation among negroes in connection with the disease in question. Dr. B. informed me that he had examined one thousand negroes without finding a single example of trachoma among them. But it would be highly important and interesting to investigate the conditions under which these people live. Whatever influence race may exercise there can be little doubt that geographical, or, better perhaps, climatic causes and unfavorable hygienic surroundings play a still greater part in the etiology of this disease. Trachoma has been styled "*the Celtic disease*"; but here, too, we should consider the conditions just alluded to.

Sæmisch[2] directs attention to the increase in the extent of trachoma along the Rhine, from its source to its mouth. Although this disease is said to almost never originates in Switzerland, we should refer this immunity to the climatic rather than the mountainous condition of the country, because mountainous regions do not furnish any protection against the spread of the affection.

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[1] Report of the Fifth International Ophthalmological Congress, 1876.

[2] Sæmisch und Græfe: Handbuch, Bd. iv, p. 66.

In Southern Russia, Mandelstamm[1] states that trachoma constitutes 35 per cent. of eye affections. And this large percentage he traces to the small and filthy homes, especially among the Israelites. There, too, *struma* and *trachoma* go hand in hand. In New York City and Brooklyn, estimated from the reports of the eye and ear hospitals of these cities during the past ten years, trachoma constitutes five per cent. of the cases of eye disease.

If I deny the existence of a bacterium in the trachoma tissue, it is still very probable that *micrococci* are the real *provokers* of the contagion, and that these should be sought in the secretion, and not in the conjunctival tissues; and, further, that a particular diathesis, and climatic and hygienic conditions, also play a very significant part in the production of this serious disease.

During the past few years the question of trachoma has been very freely ventilated, and yet, despite the prodigious amount of scientific investigation devoted to the subject, opinions are still far from being unanimous in regard to the nature of the process. By some it is maintained that in trachoma we have to do with a simple inflammatory process; while others believe that it is an inflammation with new formations, to be considered as a disease *sui generis*, with a specific course.

In order to understand the development of the structures which are considered characteristic of trachoma, it will be desirable to recall the normal condition of the human conjunctiva, and I may be excused for briefly referring to it. In the fresh state, the conjunctiva is of a delicate pink color, varying in thickness according to the part which it covers—*i. e.*, from the margin of the lid, backward, it increases in thickness, again becoming attenuated where it is reflected upon the globe; the increase in thickness being referable to the development of adenoid tissue. The conjunctival surface is not smooth; it is traversed everywhere by furrows which vary in depth, and cross each other in such a manner as to present the picture of a continuous network when viewed from the surface. These are the so-called papillæ or papilliform elevations. But this papillary condition of the surface only manifests itself to the unaided eye at that part of the conjunctiva which extends from the point of transition from conjunctiva of the lid to its point of reflection on the eyeball, known as the cul-de-sac.

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[1] *Græfe's Arch. f. Ophthal.*, Bd. xxix, 1883, p. 56.

It is easy to convince one's self of the unevenness of the healthy conjunctival surface, and that this is due to invaginations of its epithelium, and that this is most pronounced in the cul-de-sac. Reich, Sattler, Stieda, Baumgarten, Jacobson, Jun., Mandelstamm, Ræhlmann, and others agree that this condition is present in the normal conjunctiva; the point of dispute being whether these invaginations are glands or not. Stieda, Sattler, and, quite recently, Ræhlmann have shown that in the new-born there is not a trace of adenoid tissue in the conjunctiva; that beneath the epithelium there is a thin layer of fibrillar connective tissue devoid of cellular deposit; so that in a child of six months, the thickness of the epithelial and fibre-layer together should not exceed 0.05 mm.—0.1 mm.[1] In the adult the conjunctiva is 0.2 mm.—0.3 mm. thick, and in the upper third of the tarsal portion and in the cul-de-sac it may attain to a thickness of 0.4 mm.—0.5 mm.[2]; this increase in thickness being due to the development of adenoid tissue. The most recent writer on trachoma, Ræhlmann, maintains that the possibility of the development of this disease depends upon the existence of the adenoid layer, the first indication of which he found in the conjunctiva of a child of two months.

Bruch was the first writer to describe lymph follicles in the conjunctiva of the inferior eyelid of the ox.[1] Stromeyer[2] described follicles in the conjunctiva of domestic and wild animals, and he spoke of them as pathological products. Henle[3] calls them trachoma glands. Krause[4] held the lymph follicle which he found in the conjunctiva of the rabbit, fox, and birds, to be physiological formations, and considered them identical with the solitary lymph follicle in Peyer's patches in the intestinal canal. Kleinschmidt[5] and Huguenin[6] found such follicles in the conjunctiva of man. According to the observations of Waldeyer, Sattler, and Ræhlmann, lymph follicles are not present in the normal conjunctiva. Baumgarten, on the other hand, maintains that lymph follicles are also met with in the normal conjunctiva, but his evidence is not convincing. Goldzieher[7] has recently published some observations on the occurrence of lymphoid growths in the conjunctiva, from which he concludes that lymph follicles are normal constituents of the conjunctiva; but Goldzieher, it seems to us, fails to sustain his argument against Waldeyer and Sattler. Now, if the follicle be a pathological product in the conjunctiva of man, what proof have we of its presence as a physiological formation in animals. A few years ago when I occupied myself with the study of the conjunctiva of pigs, birds, and rabbits, I was not successful in demonstrating the presence of follicles in every instance, even after a very careful search and with the assistance of others more experienced than myself, and I have since come to believe that the presence of this formation in the conjunctiva is directly traceable to the manner in which these animals are cared for, and that in them as well as in man it is a pathological product. It is at least found chiefly among such animals as rabbits and pigs, which are herded and come in bodily contact, whereby opportunity is offered for the spreading of the disease through infection and direct transfer, while in birds and cats it is seldom met with.

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[1] Ræhlmann: *Græfe's Archiv für Ophthal.*, Bd. xxix., Abth. ii., 1883, p. 74.

[2] *Ibid.*

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[1] Bruch: *Zeitschrift für Wissenschaftliche Zoölogie*, Bd. iv., p. 297.

[2] Stromeyer: "Beitrage zur Lehre von der granulösen Augenkrankheit."—*Deutsche Klinik*, Heft 25, 1859.

[3] Henle: "Eingeweidelehre," Bd. ii., p. 732.

[4] Krause: "Anatomische Untersuchungen," Hannover, 1861, p. 154.

[5] Kleinschmidt: "Ueber die Drüsen der Conjunctiva."—*Græfe's Archiv f. Ophthal.*, Bd. ix., Ab. 3, p. 157.

[6] Huguenin: "Ueber die Trachomdrüsen oder Lymphfollikel der Conjunctiva," Zürich, 1865.

[7] Goldzieher: *Hirschberg's Centralblatt f. Augenheilk.*, Sep., 1882.

Synonyms : Granular conjunctivitis, which seems to be the most appropriate term to apply to this condition is also known as *trachoma*, *ophthalmia trachomatosa*, *granulations* of the *eye-lids*, *ophthalmia Ægyptica*, *O. militaris*, *O. bellica*, *O. des armées*, *O. contagiosa*, and the modern addition, *conjunctivitis follicularis*. In its etymological sense, *trachoma* (from τραχωμα, a roughness) can be applied to any of the diseases of the conjunctiva which render the surface of the latter uneven. In view, therefore, of the follicular and granular processes being classed together, and as only a certain percentage of these cases result in retractions of the palpebral conjunctiva, the term trachoma would apply more appropriately to them, but this would not be so readily understood as the designation, conjunctivitis granulosa in stadio cicatriceo.

When we examine the text-books on ophthalmology and ophthalmological journal literature, we are perplexed by the laxity in regard to the definition of trachoma from a clinical standpoint. One author designates as "*trachoma*" what another describes as "*follicular catarrh*." Schweigger[1] describes as "trachoma granule" an accumulation, immediately under the epithelium, of lymph-cells without, as a rule, any enveloping capsule of connective tissue. Arlt[2] considers trachoma identical with the chronic blennorrhœa of the conjunctiva in ophthalmia Ægyptica, militaris, contagiosa, and "granulations." Others speak of granulations when they have to do with so-called "follicular formations," which they designate as "follicles" or "granulations." Others, again, regard granulations as papillary proliferations. Sæmisch[1] considers the so-called trachoma granule as distinct from follicles as well as proliferated papillæ.

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[1] Schweigger: Text-book. Translated by Porter Farley, M.D., p. 276.

[2] Arlt: "Klin. Darstellung der Krankheit des Auges," 1881.

A large number of the older writers regarded the trachoma follicle as a normal, a physiological formation: Krause,[2] Huguenin,[3] Frey,[4] Hessling,[5] and Kleinschmidt.[6] More recent writers, among whom may be mentioned Sæmisch and De Wecker, deny that follicles occur in true trachoma. On the other hand, a great number of most competent observers regard the follicle as the basis of trachoma: Decondé,[7] Bendz,[8] Stromeyer,[9] Blumberg,[10] Horner,[11] and others. Again, regarding the nature and structure of the trachoma follicle, opinions are very divergent; some writers holding that the trachoma follicle is identical with the closed follicle of the intestine, and consists of a glandular formation with a closed enveloping membrane: Henle,[12] Stromeyer.[13] On the other hand, Mandelstamm,[14] Remy,[15] Schweigger,[16] De Wecker,[17] and others declare that the trachoma follicle has no enveloping membrane. Sattler[1] objects to the view held by Sæmisch, regarding the new-formation of lymph follicles in the conjunctiva, in his so-called follicular conjunctivitis, on the ground that it is without an analogue in pathological anatomy. Nevertheless, we do find examples of the pathological appearance of follicles in places where they never occur in a normal condition (Virchow and Bœtcher[2]), and analogous in structure to the follicles of the intestine—*i. e.*, in the liver in leuchæmia; and Birch-Hirschfeld[3] alludes to the circumstance that the hyperplastic lymphomata are not infrequently complicated with the development, in the connective tissue of different organs, as the liver, kidney, etc., of small nodes, corresponding in structure to lymph-gland tissue, and that these secondary lymphomata are seen in variola, scarlatina, leuchæmia, and pseudo-leuchæmia.

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[1] Græfe und Sæmisch: "Handbuch der gesam. Augenheilkunde," Chap. iii.

[2] Krause: "Anat. Untersuchungen," Hannover, 1861, p. 154.

[3] Huguenin: "Ueber die Trachomdrüsen oder Lymphfollicle der Conjunctiva," Zürich, 1865.

[4] Frey: Handbuch, p. 695.

[5] Hessling: "Grundzüge der Gewebslehre," Leipzig, 1866, p. 220.

[6] Kleinschmidt: *Græfe's Archiv f. Ophthal.*, Bd. ix., 3, p. 157.

[7] Decondé: Annales de la Société de Médecine de Gand, Juin, 1840, p. 122.

[8] Bendz: Compte-rendu Congrés d' Ophthalmologie, Paris, 1858, p. 236.

[9] Stromeyer: *Deutsche Klinik*, 1859, Bd. xi., p. 246.

[10] Blumberg: *Græfe's Archiv f. Ophth.*, Bd. xv., 1, p. 142.

[11] F. Horner: Die Krankheiten des Auges in Kindisalter in Gerhardt's Handbuch der Kinderkrankheiten. Bd. v. Abth. 2. p. 305.

[12] Henle: *Zeitschrift f. ration. Med.*, Bd. viii., p. 227.

[13] Stromeyer: *Deutsche Klinik*, 1859, Bd. xi., p. 246.

[14] Mandelstamm: *Graefe's Archiv f. Ophth.*, Bd. xxvii., 3, p. 106.

[15] Remy:

[16] Schweigger: *Loc. cit.*

[17] De Wecker: Text-book. Translated by L. Forbes, M.D., p. 82.

We see, therefore, a large number of distinguished observers unwilling to agree with one another as to the actual nature of trachoma, and in consequence of this disagreement there can be no certainty in regard to prognosis, and the treatment must be empirical.

THE NATURE OF TRACHOMA.

The writer's observations are based on the examination of forty-seven cases of trachoma. In every case several of the prominences of the conjunctiva were excised, and in several instances the entire conjunctiva was examined after death. In all cases, at first, the epithelial surface was found smooth, not thickened. The adenoid tissue was infiltrated with lymphoid cells, but the latter were not uniformly distributed, but in places it extended deeper into the tissue and was more closely arranged. At first there is a circumscribed arrangement of lymphoid cells, as represented in fig. 1,[1] and is then without a limiting membrane. The tissue between the foci of infiltration may be quite normal, or, at most, slightly hyperæmic. As a rule, at an early period of their existence the foci of infiltration have a round contour, and the cells at the periphery are smaller and more closely packed together than in the interior, but there is as yet no true anatomical boundary. Under sufficient power, the interior of the young follicle has a distinct reticulum. It is a framework consisting of abundantly branching pale fibres with gangliform swellings, and in places stellate,[2] branching points with pale, oval nuclei. At this early stage, therefore, the follicle has an envelope only in the sense that the cells are most closely aggregated at the periphery. At a later period the small cells at the boundary assume an oblong shape, and look like a string of beads; and finally the fibrous boundary of the follicle is formed by the cells at the periphery being drawn out into fibres; however, this fibrous boundary is not always found symmetrically developed, but some part of the circumference is bridged over with small round cells, and frequently the follicle is found to communicate with the surface of the conjunctiva through a breach in the enveloping capsule. The young follicle represents a pretty solid nodule, and cannot be easily emptied of its contents by puncture from the conjunctival surface. Leber,[3] on the contrary, maintains the opposite view. As the disease advances, the fate of the follicle will be varied according to the intensity of the inflammation, and its contents may become obliterated or undergo softening.

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[1] Sattler: Bericht über die Heidelberger Ophthal. Gesellschaft, 1881, p. 19.

[2] Virchow and Bœtcher: *Virchow's Archiv*, Bd. xviii., p. 22.

[3] F. v. Birch-Hirschfeld: "Lehrbuch d. path. Anatomie," 1877, p. 131.

Frequently the follicles are confluent, and when this condition extends over a considerable space, the typical boundary described above will not be found. Sclerosed, thickened vessels, as found by Sattler, in the interior of the follicle are developed into fibrous connective tissue; and he alluded to this as the most important circumstance in the connective-tissue transformation. According to Ræhlmann this transformation into connective tissue as issue of the follicle formation in trachoma is not rare, but he states that another fate awaits the follicle which is the more frequent —*i. e.*, the formation of *follicular ulcers.* When the conjunctival cells undergo mucous degeneration, the conjunctiva assumes a soft, gelatinous condition—the gelatinous (*sulziges*) trachoma of Stellwag. In recent trachoma there is often a vascular network about the follicle. Iwanow[1] and Mandelstamm[2] deny the existence of blood-vessels in the interior of the trachoma follicle. According to others it has a few blood-vessels. Blumberg,[3] Sattler, Horner, Stellwag, Leber, Jacobson, Jr., and Ræhlmann[4] have shown that the follicle does contain blood-vessels; and injected eyes of animals prove the existence of blood-vessels. I have repeatedly seen vessels, and quite large ones, in the interior of trachoma follicles—fig. 1. The vessels are generally found just inside the wall of the follicle, but only a few firm capillary vessels project into its interior, and these branches are so small that they are only visible by high power. *In the neighborhood of the follicle blood-vessels are often numerous, but their presence and number will depend upon the stage and duration of the disease.*

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[1] I am unable to furnish the wood-cut in time for the printers, but on page 31 of Græfe and Sæmisch's "Handbuch," vol. iv., will be found a similar picture.

[2] Ræhlmann: *Græfe's Archiv*, Bd. xxix., 2, plate v., fig. 13; also Jacobson, Jr.: *Ibid.*, Bd. xxv., 2, p. 169, plate v., fig. 7.

[3] Leber: Bericht der Ophthal. Gesellschaft, Heidelberg, 1878, d. 12.

Some observers have found the follicle of the conjunctiva surrounded by a lymph-sinus. Frey[5] and Wolfring[6] have described and pictured lymph-vessels in the periphery of the follicle of dogs; and C. Langer[1] has shown in injected preparations, that the follicle is in intimate relationship to the roots of the veins, in that the follicular substance is disposed about these roots.

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[1] Iwanoff: Bericht der Ophthal. Gesellschaft, Heidelberg, 1878, pp. 19 and 21.

[2] Mandelstamm: *Græfe's Archiv*, Bd. xxvii., p. 106.

[3] Blumberg: *Græfe's Archiv*, Bd. xv., p. 143.

[4] Ræhlmann: *Græfe's Archiv*, Bd. xxix., plate iv., fig. 7.

[5] Frey: *Virchow's Archiv*, Bd. xxvi.

[6] Wolfring: *Græfe's Archiv*, xiv., 3, p. 181.

When the follicles are isolated the tissue-changes are generally limited to the site of the follicle and its immediate vicinity, and the interlying tissue may be free from changes. But generally we have to do with various conditions of catarrh, and these inflammatory changes take place in the surrounding tissue and complicate the process.

Ulceration and cicatricial formations are a more frequent issue of the follicular conjunctival inflammation than induration through new connective-tissue formation, and the two processes generally occur together—ulceration and cicatricial formation on the surface, and connective-tissue sclerosis in the deeper parts. We have also analogous results in inflammation of other mucous tissues, *i. e.*, throat, mouth, intestines, and vagina. The scars which develop on the surface of the conjunctiva are frequently radiate, and sometimes pigmented. As a consequence of the connective-tissue formation in the deeper parts of the conjunctival membrane, the surface of the latter assumes a characteristic appearance over all that portion previously occupied by the follicles; then we see a cicatricial surface with thin epithelial covering, and the gland and epithelium are so far affected as to surrender their power to act as physiological organs. And thus by ulceration of the follicles and the formation of connective tissue in and about the follicles the entire adenoid tissue invaded by the follicular formations is destroyed. So long as the adenoid tissue remains uninvaded, it serves a useful purpose in preserving a certain degree of softness to the conjunctiva; but when its tissue is destroyed in the manner just referred to, we have a hard, irregular, unyielding, dry scar surface. This is of course the extreme result of the trachoma process, and while these changes may be brought about in a few months in acute cases of the disease, in the majority of instances years are required for the chronic form to reach the same. In chronic trachoma, the follicles are at first less dense, but aggregated in isolated heaps, and as some of these follicles are obliterated, others take their places; and so the creation of new follicles and connective-tissue formations and sclerosis goes on, so that in the chronic cases the latter changes attain to an unusual degree. The action of jequirity finds a partial explanation in the assumption that the existing follicles are destroyed, and the further development of others prevented, by the serous infiltration of the conjunctival tissue which it occasions. At least we can explain the occasional enormous thickening of the conjunctiva by the continuance of the connective-tissue formation about the new and old follicles, the latter being crowded into the tissue by the new growths, the connective-tissue formation going on, if not arrested by natural or artificial agencies, so long as adenoid tissue remains. I do not mean to maintain that trachoma produces these extreme changes in the whole conjunctiva in every instance, even when the disease is left to itself. The most common issue, when the disease is appropriately treated, is that the trachoma process is confined to a few places in the conjunctiva, and when the stage of scar formations is reached, the disease ends. In the great majority of cases, however, the process goes on.

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[1] C. Langer: *Wiener med. Jahrbücher*, 1878, p. 336.

The hypertrophic cock's-comb-like fold of the cul-de-sac has recently been classed by Stellwag[1] among the amyloid tumors. He maintains that his gelatinous trachoma (*sulziges Trachom*) is identical with amyloid tumors,—*i. e.*, that the latter proceed from the former. He defines amyloid tumors as polypoid swellings; that they are partly clodded masses which, in later stages of the process, always rest upon a cicatricial retracted base or upon an actual pedicle, and, as a rule, spring from the semilunar fold or caruncle. In support of his assumption that these amyloid tumors proceed from trachoma, Stellwag adduces that he saw them "appear and grow on conjunctivas which were unquestionably the seat of trachoma." But this is certainly no proof that they owe their existence to trachoma, and more substantial evidence is necessary to establish the etiological connection. Kubli[1] observed in a series of thirty cases of these tumors in the conjunctiva, fourteen in which antecedent or accompanying trachoma could be excluded; so we see these amyloid tumors do occur independently of trachoma.

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[1] Stellwag von Carion: "Abhandlungen aus dem Gebiete der prak. Augenheilkunde," 1882. Published by Braumüller, in Vienna.

The adenoid, epithelial invaginations of the conjunctiva in trachoma were first described by Berlin and Iwanow, and declared by them to be new growths and pathognomonic of trachoma. It is generally conceded that these structures exist, but respecting their significance opinions diverge. Reich[2], de Vincentis[3], Ciaccio[4], and recently Baumgarten, have shown that these glands of Berlin and Iwanow are physiological structures.

At birth no adenoid tissue exists in the human conjunctiva; it develops in old age. Stieda[5], Sattler[6], Waldeyer[7], deny that glands exist in the human conjunctiva, and refer these formations to invaginations of the epithelial surface at the base of the furrows on the surface of the conjunctiva. I have never found actual tubular glands in the human conjunctiva; but if we regard the loop-like invaginations of the epithelium as glands, then we may admit the existence of Stieda's tubular glands; then, also, we might with equal justice call the epithelial furrows glandular organs; if not, then the invaginations are just as little to be regarded as glands as the furrows themselves, whose complete analogue they represent. Jacobson[1] and, quite recently, Ræhlmann[2] are also of this opinion. We must show that longitudinal as well as transverse sections of the conjunctiva show round sections of such blind sacs, which may be mistaken for the lumens of glands. Mandelstamm[3] describes tubular glands which run more or less parallel to the surface of the conjunctiva. But are these not transverse instead of longitudinal sections?

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[1] Kubli: *Archives of Ophthal.*, 1882, vol. xi., No. 2, p. 167.

[2] Reich: *Græfe's Archiv f. Oph.*, Bd. xxii., p. 1.

[3] De Vincentis: *Nagel's Jahresbericht*, Bd. iv., p, 247.

[4] Ciaccio: Quoted by Henle in his "Handbuch der Anatomie des Menschen," Bd. ii, p. 731.

[5] Stieda: *Archiv f. mikr. Anatomie*, Bd. iii., p. 357.

[6] Sattler: *Græfe's Arch. f. Oph.*, Bd. xxiii., 4, p. 1.

[7] Waldeyer: Græfe u. Sæmisch, "Handbuch," Bd. i., p. 240.

The histological structure of Sæmisch's trachoma granule (Græfe and Sæmisch's "Handbuch d. Augenheilkunde," Bd. iv., fig. 2, p. 31) is not distinguishable from proliferated papillæ, and there is no justification for designating these structures as having specific qualities. The greater the proliferation of the papillæ the greater will be the number of these so-called trachoma glands. Now in trachoma these glands, originally running vertically to the surface of the conjunctiva, may, in consequence of the swelling of the parenchyma of the mucous membrane, be pushed out of their original position and extend diagonally to the surface; then, of course, transverse sections would show round sections of these loops or pouches. The analogue of this condition is found in the intestine. Ziegler[4] has shown that in the mucosa of the intestine the glands of Lieberkühn, in the neighborhood of Peyer's patches, which run perpendicularly to the surface of the mucous membrane, in consequence of infiltration and swelling of the follicles in abdominal typhus are forced out of place, so that in vertical sections of the membrane round sections of the glands are furnished. Now, if these so-called trachoma glands are the analogues of the epithelial fissures and invaginations which occur in the normal condition, then they cannot be pathological new-formations. Alongside of these invaginations, in trachoma, numerous actual interspaces sometimes occur in the conjunctiva, which are new-formations in the sense that by the swelling of the tissue the conjunctiva is folded in places. Now if ulceration of the apposed surfaces of the folds of the conjunctiva take place and in places grow together (fig. 4), the furrow may become constricted, and the development of partly communicating blind sacs give rise to the formation of cysts,[1] and in consequence of this ulcerative process the walls of the interspaces partly or wholly grow together, and in that way gland-like divisions occur in them (Mandelstamm). This tendency to cyst-formation is evidently accidental and not characteristic of the trachoma process; they are said to occur also in chronic blennorrhœa (Jacobson, Jun.). They nearly always occur at a late period of the disease. These cysts contain, so long as the epithelium is preserved, soft, tallow-like masses in which are epithelial cells, the latter being in places hyaline. Later the cysts contain a yellowish-white, pus-like fluid with few cellular elements, and of which they are readily emptied. This is a degenerative process; but sometimes a diametrically opposite condition obtains—*i. e.*, proliferation of the conjunctival epithelium; not a general uniform increase of the epithelial cells, but a localized proliferation, giving rise to inequality of the surface. In regard to the proliferation of the epithelium inward into the tissue of the conjunctiva, which is said to be more frequent than the opposite condition just referred to, although I have seen it I can add nothing new from personal observation. Ræhlmann[1] refers this condition to ancient cases of trachoma in the regenerative stage, where defects have occurred in the surface of the conjunctiva, and these defects occur principally at those places where follicles have ruptured; he, therefore, refers those solid epithelial ingrowths to regenerative proliferation of the epithelium to cover loss of substance after ulceration and excoriation.

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[1] Jacobson, Jun: *Græfe's Archiv f. Oph.*, Bd. xxv., 2, p. 139.

[2] E. Ræhlmann: *Græfe's Archiv f. Oph.*, Bd. xxix., 2, p. 113.

[3] Mandelstamm: *Græfe's Arch. f. Oph.*, Bd. xv.

[4] Ziegler: "Lehrbuch der path. Anatomie," Jena, 1881, pp. 652–655. See also Cornil and Ranvier: "Manual d' Histologie Pathologique," vol. iii., p. 828.

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[1] Iwanoff: Bericht der Heidelberger oph. Gesellschaft, 1878, p. 18, figs. 3 and 4.

So far we have sketched follicular and trachomatous conjunctivitis as two conditions, but we have mainly tried to disprove certain propositions without indicating the true pathogenesis of either disease.

Actual trachoma bodies certainly do in rare instances disappear spontaneously, and such cases have undoubtedly been responsible for the confusion which exists in regard to trachoma, and have given rise to the assertion that special forms of the disease exist. But we should never end if we attempted to enumerate all the classifications which have been offered regarding granular conjunctivitis.

Sæmisch,[2] de Wecker,[3] and Preuss[4] consider trachoma-granulations as new-formations of a specific character, completely distinguishable on the one hand from true follicles, and on the other hand from papillary proliferations. Sæmisch considers the closed follicle as the pathological basis of "follicular catarrh." This is contrary to the experience of others, and an examination of the sections which I have obtained from conjunctivas in different stages of the trachoma process, would contradict this proposition, and show that the "*follicile*" *is a very faithful attendant of trachoma.* That which has been denominated follicle, both in trachoma and follicular catarrh, is an accumulation of lymphoid cells of a circular or oval, and sometimes irregular shape. We have found these structures a pretty constant product in all forms of trachoma. This means that the character of the conjunctival affection, in the one case innocent, in the other serious, does not depend upon the presence of the follicles. Just as the diffuse infiltration of lymph cells in the tissue of the conjunctiva is only a product of inflammation, so is the follicle due to a like cause; its further history depending on various accessory circumstances, such as intensity and duration of the inflammation and the power of resistance in the tissues, the presence or absence of the follicle having nothing in itself characteristic of the disease. The arrangement of the lymphoid cells into clusters or follicles is a peculiar circumstance for which I know no satisfactory explanation. There is certainly no justification upon pathologico-anatomical grounds for distinguishing trachoma from follicular catarrh, and inasmuch as the majority of histologists agree that the follicle does not exist in the normal human conjunctiva, and as we have seen that no other anatomical condition is characteristic of trachoma except the follicle, we may, with good reason, define trachoma as a follicular disease.

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[1] Ræhlmann: *Græfe's Archiv f. Oph.*, Bd. xxix., 2, p. 130.

[2] Græfe u. Sæmisch: "Handbuch der Augenheilkunde," Bd. iv.

[3] De Wecker: Text-book.

[4] Preuss: *Berliner klin. Wochenschrift*, 1869, Nos. 42, 43.

The mixed trachoma of Stellwag is nothing more than a transition stage; or, as Schmidt-Rimpler[1] says, there exists only a quantitative, no qualitative, difference between follicular catarrh and trachoma.

It is difficult to reconcile the statement of Sæmisch in regard to the differentiation of the follicle from papillæ. This writer[2] furnishes a drawing of the "trachoma granule," and says that it is distinguished from papillæ in that it is a prominence of the mucous membrane, the interior of which is infiltrated with lymphoid cells; and from the follicle in that the latter is a roundish, semicircular structure, sharply circumscribed beneath the level of the conjunctiva. But he says, although the histological elements of the "granulation" and those of the swelled papillæ may be classed alike, not alone are form and appearance of the prominences in each case distinguishable the one from the other, but that the structure—the formation—of the granulation is essentially different from that of the papillæ. *L. c.* p. 45. There is, then, according to Sæmisch, a specific trachoma granule. I have never been able to confirm this proposition. In those cases in which portions of the conjunctiva were excised, which microscopically presented the appearance described by Sæmisch, of a round, grayish-red prominence, especially in the region of the posterior fold of the conjunctiva, they were found microscopically to consist either of proliferated papillæ which had coalesced or rather grown together, or of follicles.

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[1] Schmidt-Rimpler: *Nagel's Jahresbericht* Bd. vi., p. 211.

[2] Græfe und Sæmisch: "Handbuch der Augenheilkunde," Bd. iv., 2, p. 42.

There is a vesicular formation on the conjunctival surface which some writers have described as "vesicular granulations," and which has frequently been stated to be premonitory of granular conjunctivitis. Sæmisch aptly describes them as "small, semi-globular, nearly transparent prominences, not quite half the size of a pin's head, which occur most frequently on the conjunctiva palpebræ inferioris, between the posterior fold of the conjunctiva and the free border of the lid, and principally toward the external commissure." These formations are also frequently seen beneath the upper lid, near the external commissure, as an isolated cluster of small vesicular bodies surrounded by swelled, red conjunctiva, the rest of the conjunctiva being apparently normal or but slightly hyperæmic, and we know that this condition may remain unchanged for a very long time, and without giving rise to any marked symptoms. These vesicles are generally produced by ectasia or a lifting up by a watery fluid of the epithelium, and sometimes the superficial layers of the conjunctiva. If you puncture these bodies with a needle, a clear fluid escapes, and the vesicle collapses. Sœlberg Wells speaks of a vesicular condition which he describes as having escaped the attention of other authors, and which he says is premonitory of "granular ophthalmia," and that it is due to the enlargement of "pre-existing lymph follicles in the conjunctiva." But this view was held by Burkard Eble already in 1820, and Helling,[1] Gonzée,[2] and Decondé.[3] However, these authors employed the term vesicular granulations to designate "trachoma granules," and they believed that the latter developed from the vessels. If one or more of these bodies, including a portion of the surrounding swelled conjunctiva, are removed with the scissors and hardened in alcohol, you will see in horizontal sections that these bodies are surrounded by a uniformly thick layer of epithelium with small invaginations here and there. Beneath the epithelial layer is proliferated, partly selerosed connective-tissue, permeated by numerous lacunæ-like lymph-spaces, which are lined with endothelium, and contain a finely granular amorphous mass (Ræhlmann).

Under normal conditions, the tarsal conjunctiva is attached to the tarsus only by a loose connective tissue. In chronic cases, the follicles are collected in heaps, and as some of these deposits disappear new ones develop, and it is just in the chronic cases that connective-tissue formations and sclerosis attain to an extraordinary degree. In consequence of progressive follicular formation and associated disappearance of follicles, the adenoid tissue of the conjunctiva is thickened, and later on destroyed; on the other hand, the sclerosis process converts the loose connective tissue which separates the adenoid layer from the so-called cartilage of the lid, into a mass of dense connective tissue which cannot be distinguished from the fibrous framework of the lid. This young connective tissue undergoes sclerosis and cicatricial retraction. The loose tissue between the cartilage and conjunctiva being obliterated, or rather, incorporated with the cartilage, it is easy to understand what would be the mechanical effect of cicatricial contraction of this tissue. This retraction is generally independent of any change in the fibrous framework (cartilage); but of course the latter may, in long-continued cases, also be involved. As a rule, the cicatricial changes begin in the deeper parts of the conjunctiva, and pass thence outward.

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[1] Helling: "Praktisches Handbuch der Augenheilk.," Berlin, 1881. Bd. ii., p. 117.

[2] Gonzée: "De l' ophthalmie qui regne dans l' armée belge." Bruxelles et Paris, 1842, p. 219.

[3] Decondé: "Memoire sur differentes questions qui se rallachant à l' ophthalmie de l' armée." Annales de la Société de Méd, de Gand, Juin, 1840, p. 131.

The margins of the lids are nearly always involved in the inflammatory process, and an explanation for this independence on the one hand and constant implication on the other is furnished by the condition of the vascular supply in the conjunctiva, which, excepting a few anastomotic branches, is separated from that of the cartilage; but the conjunctival vessels are very intimately connected with the border of the lids.[1] In addition to the conjunctiva palpebrarum, that of the bulbus and the cornea is also involved at some period of the disease; in the latter instance constituting the condition known as pannus corneæ. But in the case of the cornea it is principally the epithelial layer that is involved. This condition is well shown in a drawing by Irvanoff, in Græfe and Sæmisch's vol. iv., p. 188. Here we see a subepithelial infiltration consisting of small round cells, the cornea proper not generally being involved.

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[1] See diagrams in Langer's paper: "Ueber die Blutgefässe im Augenlide." *Med. Jahrbücher*, 1878, Heft. 3, p. 333.

TREATMENT.

The presence of granulations in the conjunctiva does not furnish an immunity against other forms of conjunctivitis, and when they accompany the granulations the condition is intensified, because the secretions which in those cases in which the inflammation is absent or slight are eliminated in a great measure by the tears, lodge between the granulations, especially in the cul-de-sacs, and by decomposition furnish another source of irritation and a means of contagion. It is just here that solutions of corrosive sublimate are useful, on account of the astringent properties of the drug, for the mechanical effect of removing the secretions. Of course this decomposition implies the presence of bacterial life, because we know that without them decomposition does not take place.

If we examine the methods which have been employed for treating granular conjunctivitis from the earliest time (the disease was known to Hippocrates,) to the present day, we shall see that the efforts of all have been directed toward destroying the granulations, by a multitude of means, which collectively constitute a chapter as confusing as it is elaborate. But it would be an idle waste of time to enumerate all the substances which have been employed to combat this obstinate disease. The diversity of the methods of treatment originated from the different indications which were thought to best meet the requirements of the disease. Let me first speak briefly of caustics. Some surgeons have proposed to meet the indications by having recourse to caustics. To destroy the granulations may be correct enough, but the caustic also destroys parts we do not desire to influence by it at all; *i. e.*, when you destroy the granulations you likewise destroy the conjunctiva, and you can readily anticipate the mischief which will result when the process of repair after this cauterization is effected. There was a time not so very long ago when the solid stick of argent. nit. was quite freely employed in trachoma, and even to-day we occasionally hear of its being used. Caustics may have yielded good results in a few instances when employed with intelligence and prudence, but in an overwhelmingly large majority of the cases in which it was employed for granular conjunctivitis it occasioned a great deal of suffering to the patient, and he was not recompensed by a good result, and pretty generally another condition was substituted which was even more serious than the original trouble.

If you examine sections of trachomatous conjunctivas, you will see that the tissue of the granulations is lymphoid in its nature, and also that the vascularity is generally very pronounced, and it is to the presence of these vessels that we may justly refer the production of the new-formed cells. Astringents, we know, diminish the diameter of capillary vessels. But it is a great desideratum that the action of the astringent should be *continuous*, otherwise we may produce an opposite effect. The great clinician Trousseau said: "When it is desired to bring about atrophy with astringents, their use must be energetic and sustained."

With the announcement of the discovery of micro-organisms in trachoma tissue, came, of course, the suggestion for the employment of parasiticides; and corrosive sublimate, the recognized most effective among these, was naturally seized upon. Sattler employed it in blennorrhœa of the conjunctiva in solution $\frac{1}{20,000}$, but I am not aware that he used it in trachoma. Quite soon after the announcement of Sattler's discovery, I used corrosive sublimate in trachoma as a parasiticide; but as I investigated the subject for myself, and came to the conclusion that there were no bacteria in the trachoma tissue, I still continued to use it, but mainly for its astringent and mechanical effect. The astringency of corrosive sublimate is indeed very marked when employed in a one-fourth-per-cent. solution. From personal experience in the use of corrosive sublimate in trachoma I cannot furnish any flattering results; but its solutions remain clean and free from micro-organisms, and for this reason are commendable, especially when they have to be used by a certain class of patients who are not over careful with regard to cleanliness. But you will have to watch your patients, because the corrosive sublimate is very irritating to some eyes, even in very weak solutions of $\frac{1}{10.000}$ or $\frac{1}{20.000}$. I think the best way of applying the solution to the conjunctiva is by means of the spray. A few years ago I had the privilege of introducing this means of making applications in conjunctival affections, but there is one point, to which I made no allusion in my paper,[1] and that is, the mechanical effect of the spray in removing secretions from the conjunctiva, which, as I have stated above, is of especial importance in granular conjunctivitis, because of the collection of secretion between the granules and in the cul-de-sac, and further, because patients, when they wash their eyes, do so generally with their eyelids closed.

The sulphate of copper in crystal is perhaps the most popular and generally employed of the astringents in granular conjunctivitis. But the perfunctory way in which it is so universally used in trachoma often aggravates the disease; however, copper, argentum nitr., and subacetate of lead deserve a conspicuous place in the treatment of trachoma, but they require to be used with a great deal of intelligence. Respecting the subacetate of lead, it may be urged that it forms deposits on the conjunctiva and cornea; nevertheless, if employed in a rational manner, and in conformity to the object you wish to attain, the result will be found to be often better than those obtained from copper;

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[1] ARCHIVES OF MEDICINE, 1879.

but, on account of the possibility of its forming deposits on the cornea, it is not a remedy which should be given to patients to use for themselves. Of course, I appreciate the fact that many cases must run the gauntlet of a number of remedies.

There is another method of treatment which has undeservedly, I think, been neglected. I refer to *excision* of the cul-de-sac of the conjunctiva in granular conjunctivitis, first practised, I believe, by Richet, and subsequently and more enthusiastically advocated by Galezowski. I have done this operation in eleven cases. But it should be resorted to, I believe, only as a *dernier ressort.* In three of these cases, operated on about five years ago, in which there was pronounced pannus corneæ, the latter condition was favorably influenced by the excision, and to this day no deformity exists—*i. e.*, no entropion.

The excision is done in the following manner: Seize the superior or inferior cul-de-sac, as the case may be, draw it away from the tarsus, and excise the whole length of the cul-de-sac. Thus you *detach the cul-de-sac entire, without touching the tarsus or the conjunctiva bulbi. Of course, you should not excise the granulations of the tarsus;* the cicatrization which would follow the excision of the tarsal conjunctiva would do infinitely more mischief than the granulations themselves; and, then, the granulations on this part are much less rebellious to treatment than are those in the cul-de-sac; indeed, in the few cases in which jequirity did not produce a perfect result, the granulations had disappeared entirely from the tarsal portion, but remained in the cul-de-sac. After the excision you will generally have to employ astringents.

I come now to consider *jequirity ophthalmia.* In the number of the *Annales d'Oculistique* for July and August, 1882, Dr. de Wecker of Paris, directed attention to the curative virtues, in trachoma, of the Brazilian plant, jequirity. So far as I have been able to ascertain, jequirity is a local term used by the natives of Brazil to designate its longer botanical name *abrus pecatorius.* It is a twining woody shrub indigenous to India, but now found in all tropical countries. The plant is mentioned in the Sanskrit medical writings of Susrata, whence we may infer that it has long been employed in India.[1] It is also known as Indian liquorice, Réglisse d'Amerique, Radix abri, and Liane à réglisse. It was introduced into the Bengal pharmacopœia of 1844, and into the pharmacopœia of India of 1868. The seeds, of the size of a small pea, weigh about $2\frac{3}{16}$ gm.; they are glossy, very hard and tough, and of a beautiful bright-red color with a black spot. In India they are used for making chaplets and necklaces, and we occasionally see them used in our own country for ornamental purposes. When the infusion of the root is mixed with an alkaline solution of tartrate of cupper, red cuprous oxide is deposited after a short time.

Although it has long since been settled by experimental proof that we do not possess an agent which exercises an action on the trachoma bodies similar to blennorrhoic pus, yet we have found in jequirity one which, by its being controllable, will receive a much wider application.

To de Wecker belongs the great credit of introducing jequirity into ophthalmic practice. The claim of Moura of Brazil was too late to merit more than the remark which de Wecker made respecting that writer's communication—*i. e.*, that his (de Wecker's) presentation of jequirity to the Academy of Sciences, in August, 1882, had exercised a certain emmenagogue effect on his Brazilian confrère.

The directions for preparing the infusion, as given by de Wecker, were to pulverize thirty-two of the berries (about three grammes by weight) and macerate the product for twenty-four hours in 500 grammes of cold water; then add 500 grammes of hot water; filter immediately after the liquid cools. The patient is to bathe the eyes with this lotion three times in the day. If the resulting inflammation is severe this will be enough; otherwise the bathing must be repeated on the second day, and if necessary even on the third day, the same liquid being used. The patient must remain in a dark room for fifteen days. However, it is not necessary to adhere to these directions. The infusion may be prepared weaker and without the aid of boiling water, and the patient need not remain in a dark room while the applications are being made.

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[1] Pharmacographia: By Friedrich A. Flückiger and Danial Hanbury.

I prepare the infusion as follows: I have generally attended to this myself. The seeds are first deprived of their red husks by cracking them in a clean linen bag. I then grind them into a powder (not necessarily fine) in a small pepper-mill; then add cold distilled water or glycerine and set aside for five or six hours; then filter, and the preparation is ready for use. The watery preparation should be fresh, but the glycerine preparation, if kept in a closed vessel, will be safe and useful after several days, or even weeks. The strength of the infusion should vary according to the nature of the case in which it is employed—1%, 2%, 3%; the 5% infusion being reserved for those cases in which the changes in the conjunctiva and cornea are advanced, and in which the conjunctiva is dry and degenerative changes have already taken place. This strength may be employed in other cases in which the changes are less pronounced, but we should always proceed with caution. I apply the infusion to the inverted eyelid by means of absorbent cotton wrapped about the end of a match and dipped into the infusion, and repeat it two or three times during the day and then await results. *When the cornea is not involved I* *make a single application of a fresh one-per-cent. infusion and wait thirty-six hours before repeating it or a stronger* (two-per-cent.) *infusion.*

De Wecker[1] advises to generalize the application of the infusion in all the cases of granulations, even the less intense, but he condemns its use for the removal of papillary hypertrophy, such as is produced by chronic purulent ophthalmia; and *where suppuration already exists artificial suppuration must not be superadded.* But there is one circumstance to which de Wecker alludes which should be corrected—*i. e.*, that the cornea is not exposed to any risk during the evolution of jequirity ophthalmia. Abadie[2] states that he saw both corneæ completely destroyed by jequirity ophthalmia. However, I believe that *we can control the action of the infusion by regulating its strength, by limiting the number of applicacions, and by taking advantage of the fact that there is a period of incubation, and this I would particularly commend in those cases of trachoma in which the cornea is not affected,* De Wecker stated that purulence was an essential feature of the ophthalmia, but the sooner this error is eradicated the better, because it may lead to the most disastrous results if we persist in our applications of the infusion until purulency is produced. *Neither purulence nor membrane is an essential feature of the inflammation.* In the majority of my fifty-seven cases there was no membrane, and pure purulence existed only in a very few instances.

Of my fifty-seven cases only three were without result; seventeen were partial cures—*i. e.*, the granulations disappeared from the tarsal conjunctiva, but some remained in the cul-de-sac; in two of these cases, subsequent treatment with argent. nit. removed the granulations. Only in one instance was the use of the infusion attended with an unfavorable result. The patient, a man, æt. twenty-eight, had had trachoma for three years; both corneæ were clear. There was some mucous discharge in the morning on awaking. I treated him during two months, first with argent. nit., and then with copper, but there was no appreciable change. Flushed with the success I had had with my first jequirity case, I determined to try the remedy in this second one. I first applied a 2 % infusion which was ten days old, but it gave rise to very slight reaction, and so I ventured to try a fresh 5 % infusion. This was applied on a Saturday morning at 7 o'clock. The next day at noon the patient's eyelids were œdematous and there was considerable serous chemosis; the discharge from the eye was serous; and the patient complained of severe pain in his forehead and upper incisor teeth; there was also a profuse serous discharge from the nose, and the eyelids were tender to the touch. No further application of the infusion was made. On the third day at 9 A. M. there was a slight thick and stringy discharge of a straw color from both eyes; the lids were swollen and very firm; the chemosis was very pronounced and of a dense red color. At 6 P. M. of the same day there was a patch of diphtheritie membrane about $\frac{1}{4}$ in. square on the ocular conjunctiva near the periphery of the cornea, and the latter was infiltrated. The diphtheritic condition alarmed me, and I at once proceeded to do what I could to avert the disaster which I considered imminent. There was an extensive ulceration of both corneæ; this healed kindly in the left eye, but there remained considerable loss of tissue in the right cornea. Neither corneæ perforated. All the granulations disappeared from the palpebral conjunctiva of both eyes, but many still remained in the cul-de-sac. The patient was subsequently treated with argent. nit., and after five months he passed from treatment without any true granulations. His vision was R. E. $\frac{20}{50}$, L. E. $\frac{20}{20}$. This was the second case of trachoma which I had treated with jequirity, and it convinced me of two things—*i. e.*, first, that *the infusion should be fresh; second, that* 5 % *is entirely too strong to use in any case in which the cornea is not involved.* A distinguished ophthalmologist of this city had a still worse experience with a 5 % infusion in a case in which the cornea was clear—the latter having perforated, healing taking place with adherent leucoma. However, it would be unfair to use these two cases as evidence against the usefulness of jequirity in trachoma even where the cornea is not involved, because I am convinced that the danger may be avoided by following the directions which I shall speak of further on. In the remaining thirty-seven cases the result was complete cure. I mean by this that the granulations disappeared from every part of the conjunctiva; that the pannus, which was pronounced in the majority of the thirty-seven cases, disappeared; and the sight was improved in the majority of the cases from an ability to perceive light only, to vision $\frac{20}{100}$, and in one of these extreme cases the vision was improved after two months to $\frac{20}{70}$, and there is reason to believe that the vision may become even better in time.

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[1] De Wecker: *Annales d' Oculistique*, Mai et Juin, 1883, p. 224.

[2] Abadie: *Annales d' Oculistique*, Mai et Juin, 1882.

These results show that we have in jequirity a remedial agent of decided value, and although it does not always immediately destroy all the granulations, it diminishes, in a very pronounced degree, the pain and photophobia, and has a marked influence in clearing the cornea.

From October, 1882, to January, 1884, Coppez[1] employed jequirity in 164 cases, of which 140 had granulations often complicated with inveterate pannus, which had resisted other means of treatment. Jequirity was especially successful in old trachoma with rebellious pannus—78 cures among 140 cases. The accidents were augmentation of the opacity of the cornea, or ulceration was even produced of this part; but Coppez states that renewed application of the infusion always dispersed the corneal trouble. He had seen hypopyon ectropium of inferior lid, erythema of the face and lids; and, in the case of a young girl with trachoma, pannus and ozœna,—all three troubles disappeared simultaneously.

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[1] M. Coppez (Brussels): *Recueil d' Ophthalmologie*, Février, 1884, p. 67.

There is a point of very great importance in the use of jequirity which is in striking contrast to that of inoculation with blennorrhoic pus, and that is that with the latter you can inoculate only in case both eyes are affected, because of the danger of communicating the contagion to the fellow eye. *The discharge in pure jequirity ophthalmia does not possess the property of contagion.* I have again and again tried to reproduce the inflammation in jequirity ophthalmia by inoculating (rabbits) the good eye with secretion taken from the eye under jequirity, but always with negative results. Nevertheless we should exercise a great deal of caution when using this remedy in case one eye only is affected, and bear in mind the possibility of the infusion taking on infectious qualities through long exposure to the air. The infusion should on this account be prepared fresh, and *all infusions several days or weeks old should be discarded.* I mention this circumstance because I observed that, in one instance in which the infusion was prepared in the ordinary way and kept on the table for several days, it gave rise when applied to a rabbit's eye to a true infectious disease. But I am not sure that this infectious quality may not have been acquired, during my absence from the laboratory, from contamination of the infusion in some other way than mere contact with the air. However, I consider this fact a strong argument for using *none but fresh infusions.*

The infusion should be applied in such a way that its action will be confined as much as can be to the parts you *wish to influence, for in this manner it does seem that you will be better able to keep it under control.*

Menacho[1] (assistant to de Wecker) records three hundred cases which were subject to this treatment during the second half of the year 1883 in Dr. de Wecker's clinique. They advise its use in the following class of cases: 1. Forms of torpid ulcers of cornea, facette ulcers, and principally cicatricial keratitis. 2. Strumous pannus tenuis. 3. In conditions consecutive to parenchymatous keratitis, infiltrations, and sclerosis.

Parisotti and Galezowski[2] used jequirity in ten cases, but not with the best results. Deneffe[3] declared that jequirity is inoperative in pannus; but this statement is completely at variance with facts, for it is just in such cases that jequirity furnishes such brilliant results. There are others who have been unsuccessful with jequirity, but the evidence furnished by de Wecker, Coppez, Terson, Vacher, Gruening, and numerous others, encourages us to expect better things from it.

After a single application of jequirity infusion to the eye, there is no perceptible change for several hours; the phenomena develop at first slowly, and after four or five hours there may be only a slight increase in the flow of tears; after eight hours the hyperæmia may be only slightly pronounced, but after twenty-four to thirty-six hours the eye presents the picture of a pretty intense inflammation. The lids are swelled and matted together, and, in the case of a human being, very painful to the touch. After forty-eight hours the tarsal conjunctiva is generally covered with a more or less adherent whitish or grayish-yellow membrane. The secretion is generally muco-purulent; the palpebral conjunctiva is also greatly swelled and firm, and the ocular conjunctiva is frequently, especially in the delicate rabbit, chemotic, and the cornea is nearly always involved in these animals. On the third or fourth day the inflammation begins to recede, and after three or four weeks there remains perhaps a slight opacity of the cornea and some injection of the conjunctiva; but this condition will vary according to the strength of the infusion and the susceptibility of the animal. This is what generally obtains after a single application of the infusion in the rabbit.

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[1] Menacho: Sur les indications thérapeutiques du jequirity dans les affections de la cornée, *Revue d' Oculistique*, Jan., 1884, p. 14.

[2] Parisotti and Galezowski: *Recueil d' Ophthalmologie*, Jan., 1884, p. 31.

[3] Deneffe: *Recueil d' Ophthalmologie*, May, 1883, p. 248.

THE NATURE OF JEQUIRITY OPHTHALMIA.

At Dr. de Wecker's request, Dr. Sattler of Erlangen undertook a series of experiments for the purpose of determining the nature of jequirity-ophthalmia, and the results of his investigations have been published in the *Klinische Monatsblätter f. Augenheilkunde* for June, 1883, p. 259. Professor Hilger and Dr. Giesmann tried to isolate, chemically, the active principle of the berry, but neither of these gentlemen succeeded in doing so, and so far similar attempts have been equally unsuccessful. Sattler concluded from his experiments that the active agent in the infusion was a particular bacillus, derived from the atmosphere, and that although harmless in itself, its spores developed in the infusion new physiological qualities (*l. c.*, p. 224); in other words, that they were metamorphized from an innocent into a pathogenic bacterium. But such a transformation has not yet been proven, nor does Sattler furnish a demonstration of it. However, Sattler maintained that jequirity ophthalmia was an infectious disease. In order to solve this problem it was necessary to show that a distinct bacterium existed in the infusion, which constantly returns in all infusions of the berry, and it should be found again in the tissues which it infects, and also in the secretions furnished by the latter. It must further be shown that the infusion is without effect when the bacterium is removed or rendered inactive. Finally, the particular bacterium of the infusion should be transferred to a suitable culture medium, and rendered pure by a series of generations, and when the pure culture is applied to the conjunctiva, it should reproduce the disease. Sattler claims that his experiments justify him in answering these questions affirmatively. He maintains that the infusion contains a great number of bacilli, 0.58 mm. thick, and about 2.5 mm. to 4.5 mm. long, at times motionless, at other times moving about in a very lively way, with oscillatory or rotatory movements. These bacilli are very readily demonstrated in all fresh jequirity infusions, and there are generally other varieties of bacteria; but the bacilli of the size referred to above, are always present in greater numbers than any other forms. We must agree with Sattler so far as the presence of the bacillus is concerned, but in another direction my experiments have led me to views diametrically opposed to those of Sattler—*i. e., jequirity ophthalmia is not a bacterial disease*, and that instead of bacteria being the active agents, they, on the contrary, by giving rise to putrefactive changes, actually destroy the peculiar properties of the infusion. Let us examine briefly the evidence which contradicts Professor Sattler's view respecting the nature of jequirity ophthalmia.

If the inflammation produced by a jequirity infusion be referable to the presence of bacteria in or their action upon the infusion, then when the infusion is prepared in such a manner as to exclude all bacteria, such an infusion should be inoperative. To decide this question, I proceeded as follows: A number of jequirity berries, previously weighed, were placed for twenty minutes in a 0.1 % solution of corrosive sublimate; then they were dried with a cloth which had been kept in the sterilizer at 180° C. for two hours; the red husks were then removed with the aid of strong pincers previously sterilized by heat, two pincers being used in order to avoid touching the berry with the fingers; the berry was now crushed, or only squeezed, and dropped at once into a sterilized flask containing cold water or glycerine which had been previously subjected to 100° C. in the sterilizer during two hours; the flasks were then stopped with sterilized absorbent cotton-wool, and laid aside, at the temperature of the room, about 68° Fahrenheit, for use after from four to thirty hours. The infusions were in strength ½ %, 1 %, 2 %, 3 %, and 4 %. At the time of use, the selected infusion was examined microscopically for bacteria, and gelatine bouillon (prepared according to Koch's directions) was inoculated with the same infusion, in order to make the test complete. These infusions gave rise to the characteristic inflammation in every instance when applied to the conjunctiva of rabbits and patients who had trachoma, *a distinct period of incubation being observed in every instance;* but microscopic examination of the infusion did not reveal any bacteria, and the gelatine bouillon remained free from bacteria, although it was kept in an incubator at a temperature varying from 34° C. to 36° C. These experiments were repeated a great many times, and in every instance the most rigid precautions were observed to exclude all bacteria. According to Prof. Sattler's proposition, a pure culture of the particular bacillus found in the infusion should reproduce the characteristic inflammation when applied to the conjunctiva of animals which we know to be susceptible to the contagion; but this in my hands failed in every instance. Nor was I ever able to demonstrate the presence of the bacilli in the secretion or in the membrane formed on the conjunctiva, or in the conjunctival tissue itself: Neiser,[1] Salomonson,[2] and Klein,[3] have had similar experience. In some instances, the secretions from the inflamed eyes under jequirity did contain micrococci, but this is a common circumstance, as they are also often found under normal conditions.

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[1] A. Neiser (Breslau), *Fortschritte der Medicin*, No. 3, 1884, page 73.

[2] Dr. C. S. Salomonson, stud. med. J. Christmas Dirckinck-Holmfeld in Kopenhagen: *Ibid.*, No. 3, page 78.

[3] Klein (London), *Centralblatt für prakt. Augenheilk*, 1884, page 55.

I did not rely upon filtration for excluding bacteria from the infusion, because if the specific action of the latter were due to the presence of bacteria or to a particular bacillus, or to the latter giving rise to certain changes (chemical) in the infusion, then filtration of the liquid—*i. e.*, through clay—would, if the infusion were still operative, not dispose of the proposition that bacteria were the immediate cause of the inflammation of the eye to which it had given rise, because the infusion may have acquired noxious properties through the previous vital activity of the organism. Here I may mention by way of analogy, that the resulting infection, after inoculation of rabbits with animal tissues in various stages of putrefaction, is just as certain if the putrid substance be previously boiled and thereby deprived of living organisms, since putrefaction must be regarded, in the present state of our knowledge, as impossible without the presence of bacteria; and it is evident that in such cases sepsis was due, directly or indirectly, to the action of bacteria, because even the boiled substances used by Panum and Rosenberger, and the crystalline substance obtained by Bergmann and Schmiedeberg from putrefying beer, and which they termed sulphate of sepsin, had acquired their septic properties through putrefaction—*i. e.*, through the action of bacteria.

However, although the bacterial agency be opposed by very substantial proof, there is no evidence which contradicts the probability that the active principle of the infusion is a *vegetable substance*, and this agent is soluble in water and glycerine, and is destroyed by boiling.

Subcutaneous injections of an infusion of jequirity in rabbits kill these animals, and the death may be quite independent of any bacterial agency. I have convinced myself of this fact by repeated experiments with the infusion prepared under precautions. I should not speak of this circumstance here, but Cornil and Berlioz[1] have recently published the accounts of experiments upon rabbits and other animals, in which they injected a decoction of the berry, subcutaneously; death was produced within thirty-six to sixty hours. At the site of the injection there was an inflammatory œdema of the skin; also slight peritonitis, ecchymoses of mucosa of stomach, and tumefaction of Peyer's patches in the intestines, bacilli being found in large numbers in the blood of the heart, and all the vessels containing a great quantity of live bacteria; furnishing, in brief, the characteristic phenomena of an infectious disease. These gentlemen concluded that the bacteria of jequirity are the sole active principle. But the jequirity infusion generally contains such a variety of bacteria, that the experiments of Cornil and Berlioz only show what may be produced with many other vegetable infusions which are favorable media for certain bacteria. One of their rabbits recovered from a phlegmon and local gangrene determined by subcutaneous injection of a small dose of the decoction; they assert that it acquired immunity against subsequent infection. But in respect to this proposition we may recall Koch's reply to Pasteur's theory respecting immunity against infectious diseases, namely, that although some of the infectious diseases occur in the same animal but once, as a rule, yet no immunity is secured from others by the first attack, and adduces erysipelas, gonorrhœa, intermittent and recurrent fever, as examples familiar to all, and that anthrax not infrequently occurs twice in the same individual.

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[1] Cornil et Berlioz: Sur l' empoisonnement par le jequirity. Comptes Rendus Heb. des Séances de l' Académie des Sciences, 1883, No. 12, p. 679, and No. 15, p. 806.

I desire to express my indebtedness to Dr. T. Mitchell Prudden, Director of the Pathological Laboratory of the Alumni Association of the College of Physicians and Surgeons, New York, for many kindnesses and suggestions while conducting my experiments with jequirity and supposed bacterial trachomatous tissues.

A CONTRIBUTION OF CASES ILLUSTRATING CRANIO-CEREBRAL TOPOGRAPHY.*

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THE following cases I have had under my observation and care in Bellevue Hospital, and they illustrate, with accuracy and almost exactitude, the relationship existing between cranio-cerebral topography and symptoms. I will first read the histories of the cases I have to report, and cite a number of analogous cases, closing with a short dissertation on the relative modes of treatment, and especially the antiseptic method.

Case 1.—Michael D., aged twenty-six, Italian, a laborer, entered May 7, 1882. The history obtained was that four weeks ago he was struck over the right parietal bone with a stone. He fell senseless, but recovering went to bed very weak and with a severe headache. After ten days he resumed his work feeling as well as ever. On the fourth day after resuming his work he was obliged to cease on account of the gradual onset of paralysis involving the whole left side of the body. At first there were convulsive movements and occasional pain in both the upper and the lower extremities of the affected side. Complete flaccidity followed, and a sensation as though the parts were asleep. Coincident with these symptoms at their onset were the return of the headache over the seat of injury, and chills, now and then accompanied, as he thought, with fever. He was not delirious or comatose, and his functions were normal. On admission, the temperature in the right axilla was 99¼°; in the left, 101°. Pupils normal. He had a convulsion when he started for the hospital, and one on the following day. The spasms were clonic in character, and affected the left side of the trunk, the upper and lower extremities of the left side, the sterno-mastoid, and the left half of the occipito-frontalis. During the attack the rectal temperature rose to 104½°. He was apparently conscious during the convulsion, and certainly so immediately after it. May 9th he was operated upon by Dr. Keyes.

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* Read before the New York Neurological Society, June 3, '84.

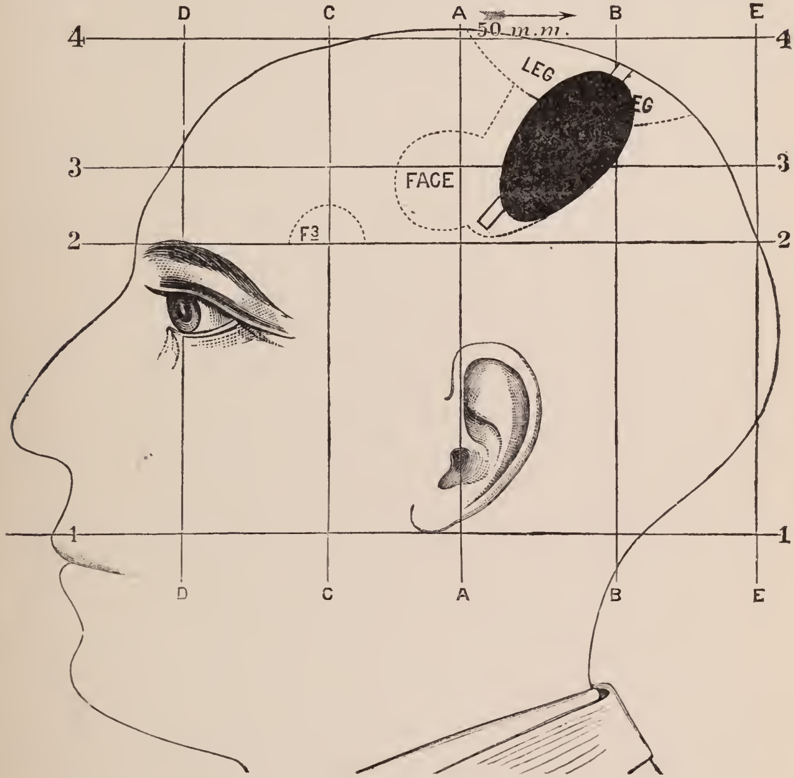

FIG. I.

A crucial incision was made over the seat of injury, the pericranium pushed aside, and the bone examined. It was found to be very much softened and highly vascular—bleeding profusely over its entire surface. Golt's trephine was applied so as to include the edge of the lesion. It was found that the degenerative process had penetrated to the inner table. The dura was hyperæmic. Exploration beneath the dura with a hypodermic syringe failed to find pus. No accurate cranial measurements were taken, but the lesion was in the left parietal bone, and over the situation of the Rolandic line. It is approximately well indicated in the accompanying illustration ; see fig. 1. The patient had two convulsions, similar in their character to those already described, within thirty-six hours after the operation. After that time they did not recur. The hemiplegia gradually disappeared, and he left the hospital cured.

Case 2.—Edward H., aged seventeen, a painter, entered June 20, 1883. He was injured a short time before by a brick falling from a building, striking him on the right side of the head. When admitted, patient was in no shock ; his pulse was full and strong, about eighty per minute. The pupils were equal and responded to light. He was conscious and answered questions intelligently, but seemed to be dazed and stupid. There was complete paralysis of the left upper extremity, partial paralysis of the left lower extremity, also a slight amount of left facial paralysis. Sensation normal. On the right side of the head there was an extensive lacerated wound of the scalp, at the bottom of which depressed bone could be easily felt. Operation was decided upon and performed by Dr. F. Lange. Extending the scalp wound toward the median line an elliptical area of bone was found depressed. From subsequent elaborate measurements made of the area after recovery I have abstracted the following brief report : "The bregma, as found by erecting on the alveolo-condyloid or base line a perpendicular passing through the external auditory meatus, was situated 19½ centimetres from the root of the nose. The anterior limit of the depression was 2½ centimetres caudad of the bregma ; the posterior limit, 7½ centimetres caudad of the bregma. As regards the lateral diameter, the median edge of the depression was 1 centimetre from the median line ; the lateral edge, 10 centimetres. From the tragus to the lateral border of the depression it was 12.7 centimetres. The depression was somewhat lozenge-shaped, 9 centimetres in its long axis, which was transverse, and 5 centimetres in its shorter axis, which was obliquely sagittal." In general terms the depression was, above and behind the ear, entirely clear caudad of the auriculo-bregmatic line. *Vide* fig. 2. The elliptical area of depressed bone was removed and the edges of the firm bone rendered smooth. A small laceration in the dura mater was discovered near the superior longitudinal sinus, but there was little hemorrhage from the wound. The remnants of the periosteum were drawn together, the slight hemorrhage controlled with ice, and the flaps sutured with catgut, two small drainage tubes, being employed. Complete antiseptic precautions were observed in the operation and in the dressing, and primary union of the scalp was obtained. The wound healed under two dressings. At the end of a week he regained power in the lower extremity, and shortly after in the upper extremity and the face. I have him under observation still and he has at no time developed other symptoms. No re-formation of bone occurred, but that was hardly to be expected considering the amount of bone removed.

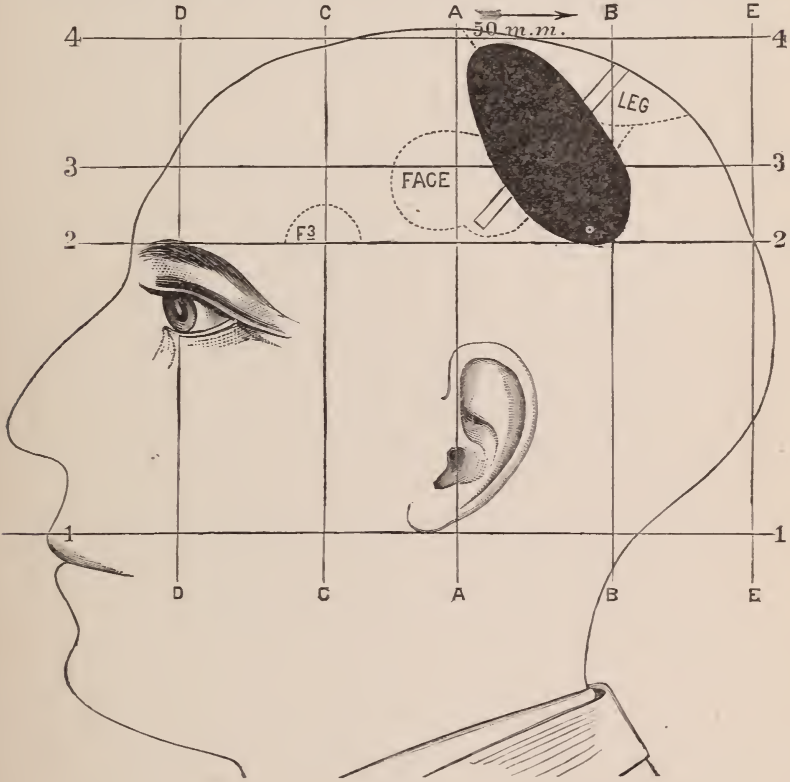

FIG. 2.

Case 3.—This case is interesting as illustrating pressure due to pus instead of bone, it being a case of compound depressed fracture without symptoms, which was trephined, followed by meningitis with the formation of pus, especially over the motor area, with the development of symptoms, first, of slight pressure causing irritation of the cortex, later, pressure sufficient to cause complete paresis of the muscles corresponding to the area involved. Henry K., aged twenty-six ; German, a laborer ; entered Dec. 22, 1882. He had been struck by a brick falling from a building, and had a compound depressed fracture of the skull slightly to the left of the median line and just posterior to the coronal suture. The area of the fracture was small, and it was slightly depressed, the inner table, however, more than the outer. He was immediately trephined, Dr. McBurney, visiting surgeon, performing the operation, removing the depressed portion. There was no wound of the dura mater. The patient did well until the sixth day, when symptoms of meningitis developed. There was a steady rise of temperature with occasional vomiting. There were first occasional twitchings of the toes of the right side ; later the leg. Then tonic contraction of the fingers of the right side set in ; this condition later giving place to complete paresis of all parts previously convulsed. His condition grew worse ; high temperature and loss of functional power supervened, the patient dying in coma.

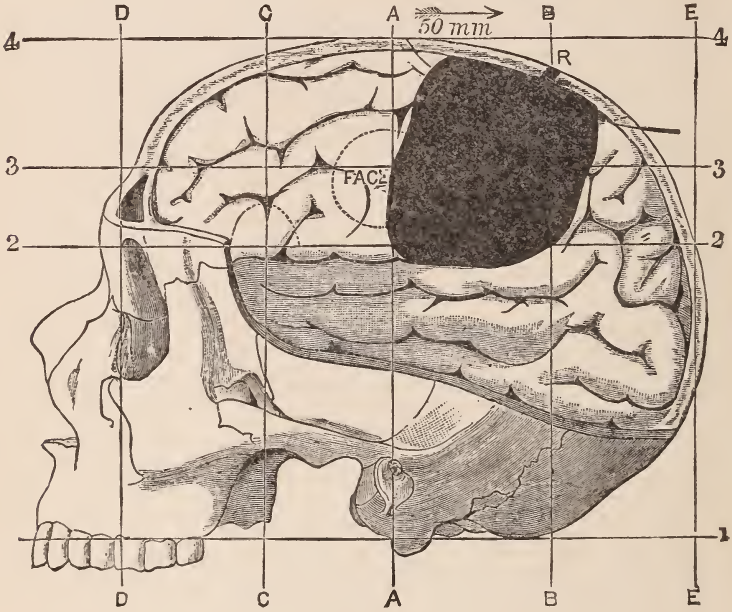

FIG. 3.

Autopsy.—Eight hours after death showed acute suppurative meningitis of the convexity on the left side. A thick layer of pus covered the left convexity and was especially deep along the fissure of Rolando. It was quite localized and deeper at this point than at its point of origin—the seat of the trephining. The pus formation involved that part of the longitudinal fissure adjacent to the paracentral lobule, but the remainder of the convexity was normal, and the interior of the brain healthy. The area of the convexity involved in the pus effusion is well shown in the appended diagram (see fig. 3). This was the only case of trephining among a large number that terminated in this manner, and the exact cause is unknown.

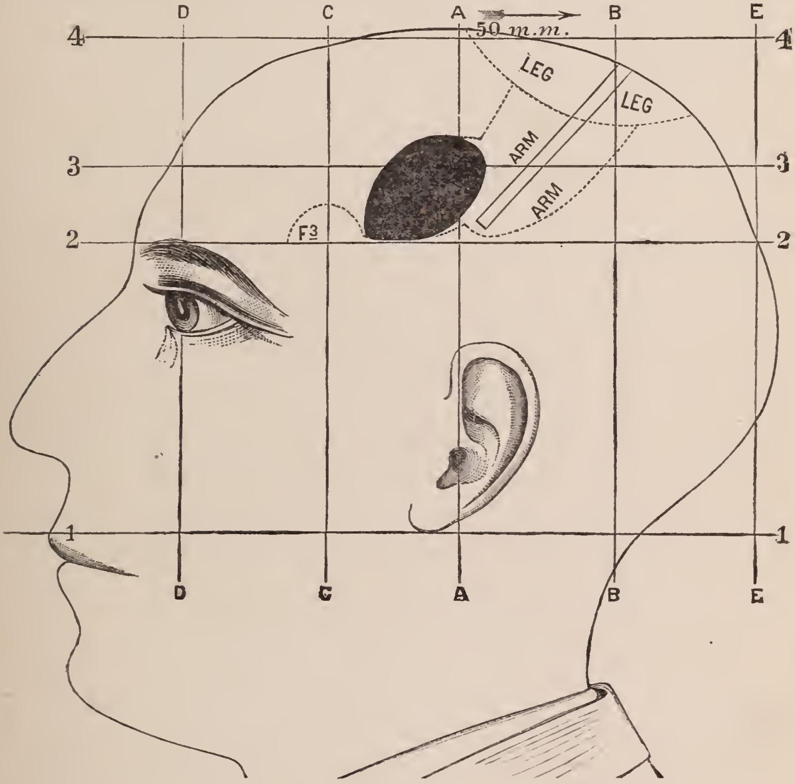

FIG. 4.

Case 4.—M. N., aged seventeen ; entered March 3, 1883. A short time before admission he fell from the seat of a mail-wagon, striking his head on a car-track. There was no wound of the scalp, but there could be easily felt a marked depression on the right side above and anterior to the ear. The patient walked into the ward, and at the time he was admitted presented no symptoms. He soon began to have twitchings of the facial muscles on the left side. This continued for half an hour when left facial paralysis supervened. It was especially marked in the lower half of the face. There was no perceptible deviation of the tongue. The pupils were even and responsive to light. There was no paralysis of the limbs, neither was sensation impaired. Acting upon the instruction of the visiting surgeon, Dr. Robert F. Weir, the house surgeon, Dr. Syms, proceeded to operate. The fracture was quite extensive, and the depressed portion was as large as a silver dollar. It was located at a point above and anterior to the ear, and on a plane slightly above the fronto-lambdoid line. Its location is well indicated in fig. 4. The depressed portion was removed and the edges of bone trimmed with the *rongeur* forceps. The dura was lacerated at one point, from which there was some hemorrhage. The patient was in considerable shock after the operation, but rallied during the night. The next day he was in fair condition, and the facial paresis began to disappear. This together with his general recovery was rapid, and on March 18th, he was allowed to get up. His subsequent recovery was complete.

These four cases can be placed, I think, in the list of contributions to cerebral topography. It is to be regretted that more accurate cranial measurements were not taken, but in these, and it is the case in the majority of cases of trephining for one cause or another, there were local indications pointing to the area to be trephined. Measurements should, however, always be made for confirmatory if not for diagnostic reasons. As far as I have been able to ascertain from published cases, there are only four on record where the operation was performed based solely upon cranial measurements. These were the cases of Broca and Lucas-Championierre, Dr. Weir's case, and one of Dr. Seguin's operated upon by Dr. Sands at Roosevelt Hospital; and the accuracy of these measurements was verified in a brilliant manner.

As regards parallel cases to the first case in this article, that of traumatic epilepsy, we have a moderately large number on record. The most complete collection is that of Mr. Walsham, to be found in St. Bartholomew's Hospital reports, vol. 19. Here are collected detailed histories of eighty-two cases, of which forty-eight were completely cured, thirteen were improved, four not improved, and seventeen died.

As regards the cause, the nature, and the situation of the head lesion to which the epilepsy was ascribed, in seventy-two out of the eighty-two cases of epilepsy, a distinct lesion standing in the relation of cause and effect was noted, the violence varying from a severe injury to a slight blow. The *nature* of the injury varied from a simple contusion to a compound depressed fracture. The *seat* of the head injury varied, but in nearly two thirds of the cases it was situated over either one or the other parietal bone. This however, I think applies to all head injuries. As regards the time of onset, duration and character of the symptoms, the convulsions appeared in a space of time varying from a few hours to several years, and the duration of the attack varied approximately.

Convulsions were generally the chief symptom. Pain was present in one half the cases. In some it was persistent, in others only excited by pressure on the scar or seat of injury.

In twenty, paralysis existed, in addition to the epilepsy, varying from partial loss of power to complete hemiplegia. In one there was paralysis of the muscles of the face. In three there was aphasia.

In regard to the existence of local indications for the seat of trephining in the eighty-two cases, there were in seventy-nine local evidences distinguishing the injured area, and, I may here repeat, it is these local lesions in the great majority of cases that have guided the operator, to the exclusion of cranial measurements. In forty-four of the eighty-two cases, there was a deep cicatrix or a depressed spot painful on pressure, sensitive, or the seat of constant pain. Pressure in two cases caused vertigo, in one case convulsions, in one case rigidity of the arm. In one the temperature was 3° F. higher than over any other part of the head. In a few cases there were sinuses leading down to dead bone.

As regards the *condition* of the parts discovered at the operation, in twenty-nine cases there was depressed bone or depression of the inner table pressing on or projecting into the brain. The dura mater in three cases was adherent, in seven it was penetrated by spiculæ of bone. In one there was a hard scar of its substance,—the result of a former wound. In twenty-one the bone had undergone alteration,—was thickened, carious, thin, necrosed, or porous. In three there was exostosis.

As regards the result of the operation and the probable cause of death in the fatal cases, in the eighty-two, sixty-five recovered and seventeen died. Of the sixty-five that recovered, forty-seven were completely cured, thirteen were improved, four were not improved. In nineteen of the forty-seven cures no fit recurred after the operation ; in eighteen there were one, two, or more convulsions in the first few days ; in eleven it was not stated, but the cure was said to be complete.

Of the seventeen fatal cases, in two the head was found healthy on autopsy. The most prominent cause of death was meningitis, of which there were seven cases. The mortality was seventeen in eighty-two, or twenty per cent. The situation of the lesion was not accurately stated, but from the appended tables the following deductions are made :

1. Where mania, idiocy, or mental deterioration occurred the lesion may be generally said to have existed in the anterior half of the cranium.

2. Where paralysis existed, the parietal bone with the exception of two cases was the part injured.

In four cases the paralysis affected the arm more than the leg, and it is interesting to note that in the only one of these in which the exact position of the lesion was given it was over the convolutions around the anterior and lower half of the fissure of Rolando. In two of the three cases in which aphasia was a symptom the surface over Broca's area was involved. In all cases with but very few exceptions there were local indications pointing to the area to be trephined.

The experience of Dr. Hammond compares very favorably with the cases collected by Mr. Walsham, presenting in addition a much smaller percentage of mortality, as in thirty cases only one died. In these thirty cases there were nine complete cures, although in two of these there was no injury of the skull to be discovered. Of the remaining twenty-one cases there was no cranial injury in three, and in eighteen cases, although there was an injury, the operation proved unsuccessful. As was stated above, one case died.

The second case in my paper—that of compound depressed fracture, operated upon on the day of injury,—illustrates the application of antiseptic surgery to cases of this description. Primary union of the scalp with complete healing under two dressings stands in strong contrast to the slow healing and homely cicatrix existing under the old method of treatment, where the wound was packed with lint soaked in some styptic or healing solution, and allowed to granulate.

In a trephining conducted according to the present antiseptic practice, the head is shaved, and the neighborhood of the wound thoroughly cleansed. Antiseptic solutions are used for the instruments, hands, etc., and continuous or frequent irrigation, generally with bichloride of mercury solution, employed.

All vessels are ligated with cat-gut, and the wound sutured with the same, first trimming its borders of all ragged integument and hair follicles. Drainage tubes may be employed or not as the operator decides. An antiseptic dressing of gauze and cotton or peat is applied, and not disturbed for a week or more, when the wound should be found healed. Primary union is almost always obtained as in ordinary scalp wounds, and is much preferable to the elevated and inverted cicatrix of the granulating wound, and in two cases I have seen necrosis of the bone result in the slow granulating wound.

In a case of my own not here cited, one of compound depressed fracture without symptoms, treated strictly antiseptically, primary union of the wound under *one* dressing-place was obtained, drainage tubes being here dispensed with.

The fourth case in my paper was a simple fracture with depression, but with symptoms indicating cortical pressure. Trephining was justifiable, and early operation in these cases I think is preferable to delaying until severe symptoms arise. This, however, is a contested point, and more proper for discussion by those whose experience has extended over a longer time than has my own.

THE NEGATIVE ARTERIAL PULSE AND NEGATIVE ARTERIAL TRACE.

BY A. T. KEYT, M.D.

THERE is a normal negative arterial pulse. The following are established physiological facts:

1. The arteries expand during systole, and return to their original calibre during diastole.

2. The arteries elongate during systole, and return to their original length during diastole.

3. The movements of expansion and return, and of elongation and return, are synchronous, and due alike to the advance and recession of the wave of blood from the heart.

4. The positive arterial pulse is co-incident with the movements described and produced likewise by the wave of blood from the heart.

5. The arteries are thrown into curvatures in systole and redressed in diastole. This rhythmic flexion or locomotion of the arteries is a necessary consequence of their elongation between fixed points; besides, it is a matter of direct observation.

With these facts, which no one will question, we are prepared to make the following deduction:

As the curvature is formed in systole the convex aspect pushes out the contiguous tissues, and the concave aspect simultaneously draws in the contiguous tissues. As the curvature is redressed in diastole, the tissues that were protruded fall back to their original place, and the tissues that were retracted rise up to their original place.

Then the integuments over an artery in which there is distinct locomotion will present two lateral spots, of which, in systole, one rises whilst the other falls, and in diastole one falls whilst the other rises.

The systolic rise and diastolic fall answer to the positive pulse of arterial locomotion, while the systolic fall and diastolic rise answer to the negative pulse of arterial locomotion.

It is apparent that arterial expansion, adding itself to arterial locomotion, while increasing the positive, tends to lessen the negative, pulse.

The aspiration we speak of is as real as arterial lengthening and lateral locomotion. There are no physical conditions to defeat it. The movement of the artery to one side tends to create a vacuum, which is immediately filled by sinking in of the skin covering the part which the artery vacates. If lateral locomotion of the arteries is accepted as a physiological fact, there is no escape from the conclusion of the existence of arterial aspiration and a negative arterial pulse.

Demonstrations.—Besides this *a priori* evidence in favor of a negative arterial pulse, it is susceptible of direct demonstration.

If in persons with visibly pulsating arteries we watch attentively the movements, we will see an unequivocal simultaneous rise of the skin at one spot and fall at another. In some persons the retraction is almost as marked as the protrusion. The phenomenon I have often seen and pointed out to others, who immediately confirmed its reality.

It may be further beautifully demonstrated by placing over the spot of systolic recession a very delicate Hérisson sphygmometer, whose rim and membrane are of small diameter (the instrument carefully placed so as to exclude the agency of impact against the rim). In this experiment the liquid column is seen to suddenly fall and gradually rise, exactly inversely with the sudden rise and gradual fall observed when the instrument is over the spot of systolic protrusion.

Another way of demonstrating the negative arterial pulse is by graphic traces. The experiment, however, is not remarkably easy, and requires for its successful performance careful manipulation with a very sensitive sphygmograph and special explorer. The difficulty lies in the fact that, when an ordinary explorer is placed to obtain the effect of aspiration, the rim, if it be a membrane explorer, or, the supporting bar, if it be a spring explorer, rests directly upon the artery and receives the effect of impaction of the pulse-wave. The effect of this impaction, as pointed out by Dr. S. E. Post, is to cause the membrane or pad of the explorer to fall and so carry the line downward in systole. In this case the line falls both from arterial displacement and elevation of the frame by the lifting force of the pulse-wave; and the trace so obtained divides its mechanism of production between the two factors. The position most favorable for aspiration is the one most favorable for impaction, and it seems impracticable to place the ordinary explorer so as to receive the former effect without the latter. The combined result is an inverted trace more ample and pronounced than could be produced by displacement alone.

However, with the delicate sphygmometer, before referred to, attached to my sphygmograph for an explorer, traces may be obtained of the pulse of aspiration entirely free from the element of impaction. The trace is very small, but so is the trace of the positive pulse taken with the same explorer. Figure A is an example of such trace obtained from a lady whose radial pulsated visibly and with distinct systolic aspiration. Doubtless if the negative trace of displacement in its purity could be obtained with the usual larger explorer the result would be more pronounced.

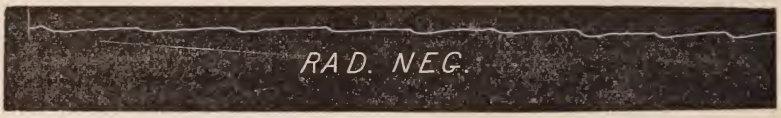

FIG. A.

With these evidences in support of the negative arterial pulse we feel justified in submitting it as a true physiological phenomenon.

THE NEGATIVE ARTERIAL TRACE.

In a late paper[2] I gave examples of inverted traces obtained when the explorer was placed by the side of the arteries instead of directly over them, and gave the demonstration of the arterial origin of these negative traces—an idea which it seems had not been previously entertained. I showed that the negative trace so obtained presented the same general form as the positive arterial; that it fell as the positive rose, and rose as the positive fell; that it presented the same number and position of secondary waves, the same aortic notch, as the positive; and that it was exactly synchronous with the positive in primary start, and each individual phase throughout. These characteristics and correspondences were considered to stamp this negative trace as produced alone by the arterial movements. (See figures.)

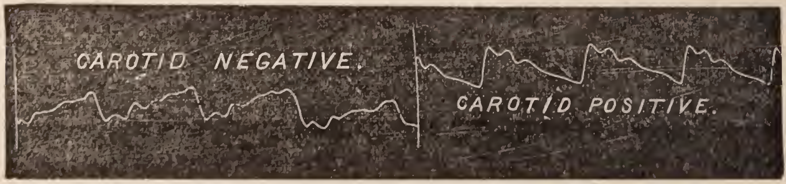

FIG. 1.

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[2] *Boston Medical and Surgical Journal*, October 11, 1883, p. 310.

In seeking an explanation of the phenomenon, I remarked that its mode of production had been to me a puzzling problem, but that I then believed its solution was found in the locomotion of the arteries. I applied the mechanism of production of the negative arterial pulse in explanation of the negative arterial trace. This theory is still correct as a factor of the mechanism, but I now admit that it is not sufficient to wholly explain the trace as ordinarily obtained; there is another important factor.

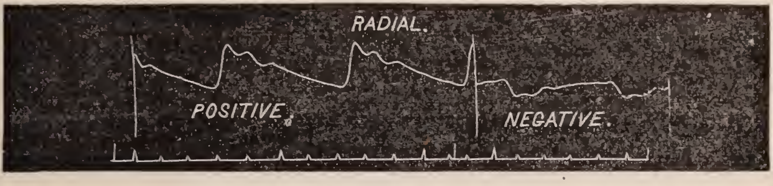

FIG. 2.

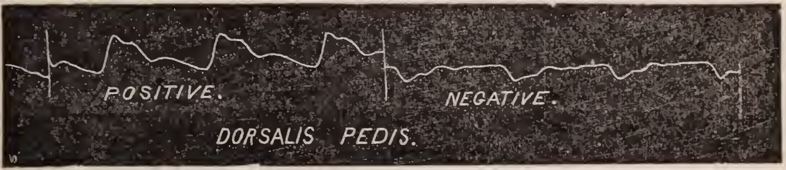

FIG. 3.

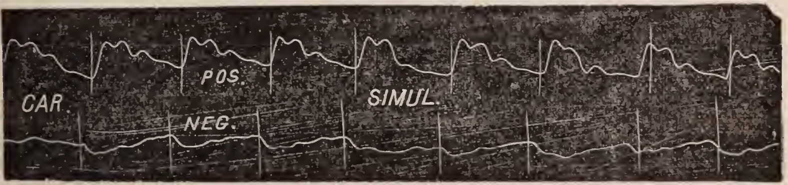

FIG. 4.

Dr. S. E. Post, after conversion from the theory of venous to that of arterial origin of this trace, has divined and pointed out the agency in its production of "direct impact against the frame instead of against the diaphragm of the explorer."[3] There can be no question that such "frame-impact" is capable of causing the diaphragm to descend in systole and ascend in diastole, and of reflecting in inverted relation all the details of the arterial pulse. I gladly accept this agency as the stronger factor in the ordinary production of the negative arterial trace.

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[3] See ARCHIVES OF MEDICINE, February, 1884.

THE THERMIC PHENOMENA IN CONTRACTION OF MAMMALIAN MUSCLES.

(Second communication from the Leipzig Physiological Institute [1]).

By ROBT. MEADE SMITH, M.D.,

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NEARLY all the information which we possess concerning the thermic and electric phenomena of muscular contraction has been acquired through the study of the excised frog's muscle; and if we could disregard the fact that every stimulation of a muscle diminishes its store of irritable matters concerned in the production of contraction, and at the same time causes an accumulation of the waste products of contraction, and to that extent reduces the potential energy of the muscle, we might hope that the study of the isolated muscles of cold-blooded animals would ultimately reveal all the conditions of their functional activity.

But we know that with every repetition of irritation there is a continuous decrease in all the muscular activities, pointing not only to a marked diminution in the supply of contractile material, but, as Heidenhain has shown, to still more complex alterations, as evidenced by the unequal decrease in the power of doing work and of heat production.

If we compare the excised muscle with the living muscle still in the circulation, we find that the latter may be subjected to a much more prolonged stimulation without losing its capability of doing work, and that in the interval of rest following the exhaustion consequent upon prolonged contraction, its energy is restored much more quickly and completely than when the changes produced by the circulation are impossible.

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[1] See Du Bois Reymond's *Archiv für Anatomie und Physiologie*, Physiologische Abtheilung, 1881, p. 105.

From the fact that the bloodless exhausted muscle is able by rest to regain a portion of its contractile force, we must conclude that in the interior of the muscle there is a store of material which is capable, even without the influence of the blood, of being converted into irritable contractile matter. And although these matters must originally be derived from the blood, a continuous contact of muscle and blood is not essential to their final development; though the comparison of the bloodless and excised muscle shows that this accumulation and development is facilitated by the blood-stream. Consequently the blood gives to the muscle a greater power of prolonged contraction, and, therefore, increases its power of doing work; while an examination of the gases of the arterial and venous blood of contracting muscles gives us some idea of the nature of the chemical changes occurring in the muscle, and the share taken by the blood in such processes.

The question arises whether the influence which we have seen that the circulation exerts on the work accomplished by the muscle also applies to the heat production in muscular contraction.

In my first communication on this subject I reported a number of experiments which seemed to prove that the bloodless muscle not only did not become as warm in contraction as the muscle still in the circulation, but also that the actual heat production was much less. The method of closing the circulation in the muscles then employed was, however, a tedious one, and necessitated the removal of the coverings from the limbs of the animal; thus, by permitting cooling of the skin, interfering with one of the requirements of the experiment. A new method of closing the circulation, suggested to me by Prof. Ludwig, ensures much purer results, and permits of greater variation in the conditions of the experiments. This method rests on the fact that when hydrostatic pressure is brought to bear on the interior of a rubber tube, its walls do not dilate uniformly, but a ball-like expansion forms at a single point; consequently, if a narrow thin-walled rubber tube, one end being closed, is inserted through the left axillary artery into the dorsal aorta, the lumen of the latter may be entirely obliterated by forcing water into the rubber tube.

To accomplish this a piece about 100 mm. long was cut out of a flexible elastic catheter, with an external diameter of about 2 mm., on to one end of which a piece of thin rubber tube about 30 or 40 mm. long was tightly fastened with thread; a hollow conical metal point was fastened in the free end of the rubber tube. Into the other end of the catheter was fastened a metal tube and stop-cock which fitted the nozzle of a small syringe. In order to facilitate the insertion of the flexible catheter, a wire stylet was passed through its interior into the metal point in the rubber tube. Half per cent. salt solution was employed to dilate the tube, the amount which could be injected without bursting the tube being always determined by measurement before the commencement of the experiment.

In order to permit of closure of the circulation through the descending aorta, the left axillary artery is exposed in a dog, when it crosses the first rib, all branches tied, and the main trunk surrounded by double ligatures. The vessel is then opened between the two ligatures, and the catheter and tube inserted, and, after loosening the lower ligature, pushed down into the descending aorta. In order to determine whether the bulb lies in the proper position, and whether its dilatation completely occludes the descending aorta, the femoral artery is connected with a mercurial manometer; if the bulb is then dilated by the injection of the salt solution so as to occlude the aorta, the pulse instantly ceases in the vessels of the lower extremity, and the blood pressure, as measured by the manometer, sinks within a few seconds nearly to zero, while the circulation continues in the head and right upper extremity. If the contents of the tube which connects the femoral artery with the manometer are pressed into the former, the column of mercury in the manometer sinks below zero and remains constant as long as the occlusion of the aorta continues perfect, showing an absolute cessation of all circulation in the vessels of the lower extremities. If the stop-cock on the catheter is then opened, the contents of the rubber tube are expelled, and instantly the column of mercury in the manometer commences to pulsate and to show a rapidly increasing blood-pressure in the femoral artery. As might be expected, however, the original blood-pressure in these vessels is not immediately regained, but only after the relaxed empty vessels regain their normal tone; this usually occurs within a few minutes, as shown by the following examples:

I.—Pressure in the femoral artery before closure of the aorta was 134 mm. Hg. The aorta was closed for five minutes, during which time the pressure was zero. Thirty seconds after reopening the aorta the pressure was 65 mm., and four minutes later was 120 mm.

II.—Pressure in the femoral artery before closure of the aorta was 100 mm. Hg. The aorta was closed for fifteen minutes, during which time the pressure remained at zero, and one minute after reopening the aorta the pressure stood at 100 mm.

Occasionally, when the closure is very prolonged, or often repeated, the pressure in the manometer will be noticed to rise gradually and slightly above the zero point, probably from the dilatation of the aorta at the point of closure, or from the expansion of the rubber tubing losing its globular form and becoming more elongated. All that is then necessary is to inject more fluid into the rubber tube when the occlusion will again become complete, and the pressure again fall to zero.

The rubber catheter was always inserted at the commencement of an experiment and allowed to remain throughout its entire course, as it was found that its presence in the undilated condition appeared to be without influence on the circulation, and coagulation was never produced even when the experiment lasted several hours.

Since, as proved by post-mortem examination, the aorta in my experiments, which were made on dogs weighing from twenty to twenty-five kilo., was always occluded about midway between the origin of the axillary artery and the diaphragm, when the closure was prolonged more than a few minutes it invariably produced symptoms of dyspnœa from the interference with the circulation of the diaphragm. This was always counteracted by artificial respiration.

In other respects the experiments were conducted in about the same manner as already described in my first paper (*loc. cit*, pp. 106, 107).

Throughout the entire course of the experiment, the animals were kept profoundly narcotized by intravenous injections of chloral solution. The thermometers employed were carefully compared with one another, were graduated into $\frac{1}{10}$° C., and by means of a magnifying-glass $\frac{1}{100}$° C. could readily be estimated with accuracy. One thermometer was inserted through the right carotid artery into the arch of the aorta to measure the temperature of the arterial blood; another was passed through a small incision in the skin deep into the furrow between the *vastus externus* and *rectus cruris* of the right thigh, to measure the temperature of the muscles. The crural nerve was divided at Poupart's ligament and the peripheral end placed on shielded electrodes connected with an induction apparatus. The condyles of the right femur were clamped to the table and a weight of five hundred grm. so fastened on to the freely movable lower extremity that contraction of the extensor muscles of the leg raised the weight and recorded the height of the lift on the moving paper of the kymographion. The entire right inferior extremity was wrapped with the greatest care with several layers of cotton batting and allowed to remain undisturbed throughout the entire course of the experiment; by this means the temperature of the skin was kept within 0.7° or 0.8° C. of that of the resting muscles. Most of the experiments were made in May, June, and July, 1883.

The first point to be determined was, whether the blood, under the conditions under which the experiments were conducted, was the only channel by which heat was removed from the mass of the muscles. That this is the case is proved by the fact that the temperature of resting muscles, as measured by a thermometer imbedded in their mass, remains unchanged by arrest of their circulation through closure of the aorta; but when the blood current is restored there is a rapid change in the muscular temperature, the character of the change depending upon the relative temperatures of the muscle and arterial blood. Therefore, before considering this point further, the changes in temperature of the arterial blood produced by arrest of circulation through the aorta must be first considered.

Examination of the thermometer whose bulb lay in the arch of the aorta showed that an invariable result of the closure of the descending aorta was a decrease of temperature in the volume of blood still remaining in circulation.

The following examples taken from four experiments made on different animals furnish illustrations of the degree and course of this reduction in temperature.

	Time after Closure of the Aorta (Minutes).						
I.	0	1	2	3	4	5	6
Temperature of blood	36.15	36.05	35.95	35.90	35.85	35.80	35.76° C.
Loss of temperature in each minute	. .	0.10	0.10	0.05	0.05	0.05	0.04° C.

Total loss of temperature in closure of aorta of six minutes' duration = 0.39° C.

II.	0	1	3	5	7	9	11
Temperature of blood	34.56	34.42	34.26	34.22	34.20	34.20	34.20° C.
Loss of temperature in every two minutes	. .	0.14	0.8	0.2	0.1	0.0	0.0

Total loss of temperature in closure of aorta of eleven minutes' duration = 0.36° C. in the first seven minutes; without change in the four following minutes.

III.	0	1	2	5	10
Temperature of blood	38.36	38.15	38.13	38.06	38.06° C.
Loss of temperature in each interval	. . .	0.21	0.02	0.07	0.00

Total loss of temperature in closure of aorta of ten minutes' duration = 0.30° C. in the first five minutes; without change in the five following minutes.

IV.	0	1	3	7	8	10	15
Temperature of blood	39.50	39.30	39.32	39.27	39.25	39.20	39.20° C.
Loss of temperature in each interval	. .	0.20	+0.02	0.05	0.02	0.05	0.00

Total loss of temperature in a closure of the aorta of fifteen minutes' duration = 0.30° C. in the first ten minutes; without change in the five following minutes.

The above experiments show that the backing up of the blood in the upper half of the body changes the normal relation between heat production and heat dissipation with a consequent reduction in temperature of the circulating blood, at first rapid but soon reaching its maximum. Even when a new source of heat is developed in the lower extremity the temperature of the arterial blood in the upper portions of the body follows precisely the same changes as detailed above, provided that all circulation between the upper and inferior portions of the body is prevented. If, however, the occlusion of the aorta be not complete and a certain amount of blood circulate through the lower extremities, a development of heat in those portions of the body will cause an elevation of arterial temperature as measured in the arch of the aorta. Thus in the last of the above illustrations, the slight increase in temperature in the third minute after occlusion of the aorta was coincident with a rise of the column of mercury in the manometer in connection with the femoral artery; when, however, a new quantity of fluid was injected into the rubber tube and the arterial pressure in the lower extremities again reduced to zero, it was found that the temporary rise was again replaced by a fall in the temperature. Indeed it was found that the temperature of the arterial blood was a very good index of the degree of closure of the aorta, an incomplete occlusion being evidenced by a rise of arterial temperature almost as soon as by the rise in the mercurial manometer.

As might be expected, when, after closure for a number of minutes, the aorta is again rendered patulous, the fall of temperature in the arterial blood is replaced by a rise; the remarkable fact was however noticed that the temperature of the arterial blood after opening of the aorta rises above its original height, even though the animal has remained motionless and in deep narcosis during the entire period of aortic obstruction. The following examples obtained from experiments on three different animals illustrate this point:

	Before closure of aorta.	At end of closure of aorta.	After reopening the aorta.
1. Arterial Temperature .	39.18° C.	39.09° C.	39.27° C.
2. " " .	37.97° "	37.70° "	38.10° "
3. " " .	39.23° "	39.05° "	39.30° "

Consequently, if the blood which has been reduced in temperature by closure of the aorta becomes heated above its original temperature by circulating through the parts which had been deprived of blood, it follows that heat must have been accumulated in those organs even in the absence of the circulation. Although not lying within the immediate scope of the present investigation, it seemed to me worth an attempt to determine whether this heat production in the bloodless organs was at all analogous to the increase of temperature which often occurs after death.

A dog, deeply narcotized with chloral, was well covered with cotton wadding, and accurately compared themometers were inserted under the skin of the thigh, between the extensor muscles of the leg, against the femur, in the vagina, and through the right carotid artery into the arch of the aorta.

	Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
Before closure of the aorta	37.67	38.30	38.38	38.25	37.97° C.
Three minutes after complete closure of the aorta . .	37.61	38.32	38.38	38.32	37.70° C.

After reopening the aorta, the animal was allowed to remain undisturbed for seventeen minutes, and the aorta again closed. The temperature-changes were as follows:

	Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
Before second closure or the aorta .	37.60	38.30	38.41	38.38	38.13° C.
Nine minutes after closure	37.10	38.37	38.48	38.50	37.85° C.
Temperature-changes	— 0.50	+0.07	+0.07	+0.12	— 0.28° C.

The temperature then began to fall in all the situations where a rise had been already noted, and in six minutes—that is, fifteen minutes after the closure of the aorta—the temperature stood as follows:

Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
36.85	38.35	38.42	38.45	37.80° C.

The aorta was then opened, and nine minutes later the following temperatures were noted :

Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
37.28	38.20	38.28	38.37	37.90° C.

The experiment therefore proves the truth of the supposition under which it was undertaken, viz.: that under the existing circumstances, when the circulation is arrested, more heat is developed in the bloodless parts than can be dissipated. When, however, the circulation is restored, this excess of heat production over heat dissipation is removed by the blood.

This experiment, moreover, throws special light on the view which attributes an increase or decrease in temperature of a given region to a corresponding change in its blood supply, as it is evident that an increased blood supply must produce varying results, according to the relations between its own temperature and that of the given region, and how unwarrantable it is to apply without qualification to the muscle results which are applicable to the skin. For with a larger supply of blood the cool skin, even though exposed to excessive and rapid loss of heat, will become warmer, while on the other hand the warmer muscles will become cooler. Consequently the conception must be erroneous, which is generally held as to the temperature changes in muscle from alteration in their blood supply after section of their nerves.

In order to permit of generalization of the results of the above experiment for the further prosecution of this subject, it was necessary to determine whether the resting muscle and muscle recovered from the exhaustion of prolonged contraction were capable, during interruption of their blood supply, of developing sufficient heat to cause an increase in their temperature. It is evident that if the tissue-change in bloodless muscles, which at best must be insignificant, is able to develop enough heat to be measured by the thermometer, it can be assumed with certainty that the locality in which the increase of temperature is measured is practically removed from all sources of cooling.

The following results, obtained from experiments on six different animals, are selected from a large number of similar observations.

	Temperature of the Resting Muscle during Closure of the Aorta.		Temperature of the Arterial Blood during Closure of the Aorta.		
	At Commencement.	At End.	At Commencement.	At End.	Duration of Closure of Aorta.
I.	34.63° C.	34.64°	34.59°	34.50°	5 min.
	34.53	34.59	34.56	34.26	5 "
	34.50	34.50	34.32	33.88	5 "
	34.31	34.34	34.18	33.79	5 "
II.	39.07	39.11	39.18	39.09	5 min.
	39.91	39.94	39.60	39.37	2 "
III.	38.21	38.27	38.06	37.95	5 min.
	38.18	38.27	38.10	37.80	5 "
IV.	36.14	36.17	36.15	35.76	5 min.
	36.19	36.21	36.10	35.80	5 "
V.	39.88	39.96	39.90	39.78	5 min.
	39.81	39.91	39.90	39.85	5 "
	40.06	40.08	40.02	39.93	5 "
VI.	39.60	39.64	39.40	39.13	15 min.
	39.80	39.87	39.20	39.20	9 "
	39.58	39.62	39.27	39.05	4 "

The great uniformity of the above results demonstrates that the resting muscles of warm-blooded animals develop heat independently of the blood current, and that in the method employed the heat developed by the bloodless muscle is but slowly given off.

In the contracting muscle, the conditions are different from those of muscles at rest; the former becomes very much warmer than its surrounding media, while it is probable that it is exposed to a much more considerable loss of heat, the extent and rapidity of the latter being capable of measurement by first stimulating either the normal or bloodless muscle as long as the temperature continues to rise, and then noting the changes in the temperature of the muscle during interruption of the blood supply. The increase in temperature will evidently vary with the mass of the muscle and the difference between its temperature and that of the surrounding media. In my experiments, dogs of about the same size were always selected; therefore, the first source of error in generalizing the results was to a certain extent eliminated, while the differences in temperature between the muscles at rest and in contraction, in the different instances, varied in the widest extremes, thus filling the second condition of the experiment.

In the following series, the muscles were first tetanized with an induction current and the circulation through the aorta arrested at the moment the irritation ceased.

	TEMPERATURE OF THE MUSCLE.			
Exp.	At Commencement of Aortic Closure.	At End of Aortic Closure.	Duration of Aortic Closure.	Increase of Muscle Temp. at End of Irritation.
3.—	39.93	39.93	2 minutes.	0.47° C.
	38.70	38.65	3 "	0.49° "
	38.90	38.88	5 "	0.48° "
	38.81	38.80	6 "	0.48° "
8.—	39.70	39.67	5 "	0.44° "
	39.96	39.93	6 "	0.49° "
6.—	36.62	36.55	6 "	0.43° "
7.—	36.77	36.66	4.5 "	0.83° "

While it may be stated from the preceding figures that, with a varying difference in temperature of from 0.49° to 0.43° C. between the bloodless muscle after contraction and the surrounding media, the muscle only lost 0.01° C. in temperature in every minute, (or 0.02° C. where the difference was 0.83° C.), this statement can only be held to give the results in a general way; since naturally with the reduction in temperature of the muscle the difference between its temperature and of its surroundings will also decrease, and consequently the loss of heat by the muscle must be hindered—if conduction to the surrounding media is the sole source of loss of heat. An examination of my figures has, however, shown me that the condition is a more complex one, for often when the difference in temperature is most marked, as at the end of contraction, the reduction of temperature is at first slow and then subsequently rapid; pointing to the fact that after contraction has ceased there may still be a subsequent development of heat in the bloodless muscle which will tend to balance the loss of heat by external conduction. Nevertheless, from the general conformity of results, it is warrantable to conclude that in each minute after contraction the bloodless muscle loses about two per cent. of the difference between its temperature and that of the surrounding media.

The share of the circulation in abstracting heat from contracting muscles was proven in my first communication by the comparison of the temperatures of contracting muscles and that of their arterial and venous blood, in which it was found that the excess of temperature of the venous blood over that of the arterial increased, within certain limits, proportionally with the increase in temperature of the muscle. This explanation is confirmed by another mode of explanation, which proves in the clearest manner the important rôle played by the circulation in the reduction of muscular temperature.

When the muscle is thrown into tetanic contraction and the circulation arrested just before the cessation of irritation, as already pointed out, the temperature of the resting bloodless muscle after contraction remains almost constant. If the aorta is then opened the muscular temperature instantly commences to fall, the rapidity of decrease being proportionate to the difference between the temperatures of the arterial blood and that of the muscle; therefore the fall which at first is very rapid becomes slower and slower as the two temperatures approach one another. Thus, for example, in cases where the muscle was from 0.7° to 0.8° C., warmer than the arterial blood, in the first minute after restoring the circulation the temperature of the muscle was reduced from 0.2° to 0.4° C.

Under exceptional circumstances when the irritation and closure of the aorta are simultaneously stopped, instead of the ordinary rapid reduction of temperature, if the muscle is only slightly warmer than the blood, say 0.2° C., the muscular temperature may increase with the restoration of circulation. This increase of temperature, which is always slight, never more than 0.02° C., may continue for a minute or more, and throws considerable light on the conditions under which the muscles develop heat, while the rarity and exceptional conditions of its occurrence prevent it from militating against the previous statements as to the usual cooling action of the blood.

Of the results obtained in the measurement of the heat developed by contracting muscles, the simplest are those acquired in experiments made on bloodless muscles; they will therefore be first considered.

The possible variations of the conditions of the experiment are the following: Tetanus may be produced in a perfectly fresh muscle, or in muscles refreshed by an interval of rest after previous contraction: the first condition when, after the necessary preparations, the circulation is arrested and the muscle at once thrown into tetanus; the second, when the muscle had already been deprived of blood and stimulated. Or, again, the circulation may be arrested simultaneously with the commencement of irritation, or a few minutes sooner, so as to insure the absence of oxygenated blood in the muscle.

The tetanized muscle can only increase in temperature up to a definite limit which varies for different muscles; and the rapidity with which this limit is attained also varies. In order to obtain a conception of the latter element of heat production, the height of the thermometers must be read off at regular short intervals of time, usually every 30 seconds.

In the following tabular statement of experiments made on six different animals, the upper row of figures of each series gives the temperature of the muscle at intervals of every thirty seconds after the commencement of the tetanus, the first figure to the left giving the muscle temperature immediately before the commencement of the irritation; where an interrogation mark (?) occurs, it means that at that time no thermometer reading was made. In the second row of each series is noted in $\frac{1}{100}$° C. the increase of muscle temperature at each half minute of the tetanus, while in the column headed "number of tetani" is noted the number of times the muscles had been thrown into tetanus before the instance whose temperature-changes are given. The last column gives the duration in minutes of closure of the aorta before the irritation was commenced.

Though not absolutely uniform the above results are sufficiently constant to permit of their statement in general terms. Immediately after the commencement of the tetanus the temperature of the muscles commences at once to rise, so that the increase in temperature acquired in the first half minute either exceeds or is about equal to that of the remaining period of contraction. Since the thermometer does not instantly acquire the temperature of its surrounding

															Total increase in temperature.	Number of previous tetani.	Duration of previous arrest of circulation.
I.	39.25	39.47 0.22	39.61 0.14	39.63 0.02	39.63 0. 0	39.63 0. 0									0.38°	0	6 min.
II.	39.26	?	39.65 0.39	39.72 0.07	39.71 - 0.01										0.46°	0	0
III. *a.*	34.64	? ?	34.94 0.30	35.01 0.07	35.06 0.05	35.10 0.04	35.14 0.04	35.20 0.06	35.23 0.03	35.25 0.02	35.26 0.01	85.27 0.01	35.29 0.02	35.30 0.01	0.66°	0	0
b.	34.59	34.78 0.19	34.82 0.04	34.86 0.04	34.92 0.06	34.94 0.02	34.97 0.03	35.01 0.04	? ?	35.06 0.05	? ?	35.11 0.05	? ?	35.11 0	0.52°	1	6 min.
c.	34.50	34.70 0.20	34.78 0.08	34.80 0.02	34.82 0.02	34.82 0. 0	34.85 0.03	34.84 - 0.01							0.34°	3	4 min.
d.	34.34	34.51 0.17	34.52 0.01	? ?	34.54 0.02	? ?	34.55 0.01								0.21°	4	5 min.
IV. *a.*	38.27	38.51 0.24	38.57 0.06	38.59 0.02	38.60 0.01	38.63 0.03	38.68 0.05	38.68 0. 0							0.41°	2	6 min.
b.	38.27	38.47 0.20	38.57 0.10	38.62 0.05	38.65 0.03	38.64 - 0.01	38.66 +0.02								0.39°	4	5 min.
V.	36.21	36.41 0.20	36.45 0.04	36.44 - 0.01											0.24°	2	5 min.
VI.	39.91	40.07 0.16	40.14 0.07	40.17 0.03	40.18 0.01	40.18 0	40.18 0								0.27°	1	5 min.

medium, the almost instantaneous rise in the column of mercury indicates that the commencement of contraction and heat production are simultaneous. After the first half-minute of contraction the increase of muscle temperature becomes more and more gradual and slow, and finally a point is reached in the duration of contraction when the temperature remains constant, when the heat production and heat dissipation by the muscle are equal,—a confirmation of the results obtained in the temperature measurements of resting bloodless muscles. And since in this instance there was not as great a difference in temperature between the muscles and the surrounding media as in the cases where measurements were made as to the loss of heat by resting muscles, and since I have seen by direct experiment that the thermometers employed were sufficiently sensitive to indicate within ten to twenty seconds changes in the temperature of $\frac{1}{100}$° C., it may be assumed that in the above cases where the thermometer ceased to indicate a rise in temperature, if any production of heat occurred at all, it could not have been more than $\frac{1}{100}$° C., within the minute.

The experiments further show in the uniformity of the results obtained in the instances where the the circulation was arrested simultaneously with the commencement of contraction, and in those where five or six minutes elapsed after the closure of the aorta before the muscle was stimulated, that the amount of oxygen remaining in the latter case in the blood within the muscle is without marked influence on the degree and course of the heat production by the muscle.

On the other hand it is evident that the frequency with which the bloodless muscle is tetanized, after equal intervals of repose in which the circulation is present, is of considerable influence on the heat production by the muscle. Thus in the observations recorded in number III. of the above series: in *a.* the perfectly fresh muscle, after arrest of circulation, was tetanized with a maximal irritation, the blood current then restored, and the muscle allowed to rest for twenty-one minutes; and in *b.* a second time irritated with maximal currents, again allowed to rest for twenty minutes as before; and then in *c.* irritated for a third time, allowed to rest for sixteen minutes; and in *d.* irritated for a fourth time. In the fresh muscle the increase of temperature was 0.66° C., in its second contraction it was 0.52° C., and in its third contraction 0.34° C., while the times required for reaching the maximum temperature were respectively seven, six, and four minutes; thus showing that a repetition of contraction of muscles deprived of their blood supply causes a reduction in the amount and duration of heat development.

Since it has been shown that, as far as measurements with the thermometer go, the muscle in the absence of the circulation gives up but little heat to its surroundings, but accumulates in its interior all the heat developed in the tetanus, if we accept Rosenthal's estimate of the specific heat of muscle, the amount of heat developed may be expressed in heat-units. Thus the specific heat of muscle being 0.825 (that of water being 1), reducing the maximum and minimum temperature of the above table to heat-units, it may be stated that one gramme of tetanized muscle may develop from 0.54 to 0.17 heat-units.

(*To be continued.*)

NEW BOOKS AND INSTRUMENTS.

Mental Diseases. By T. S. Clouston, Physician Superintendent of the Royal Edinburgh Asylum for the Insane, Lecturer on Mental Diseases in the Edinburgh University, etc. Pp. 631. J. A. Churchill, London.

This work contains over 600 pages, and is well printed. An apology is made in the preface for the appearance of another work on mental diseases when so many have recently been published, but the author believes that its strictly clinical method of treatment is what is needed by the student and practitioner. They were given as lectures at the Edinburgh University.

The work is divided into nineteen lectures. An enumeration of the headings of each lecture will give an idea of the subjects treated and the scope of the work.

Lecture 1. The Clinical Study of Mental Diseases.

2 and 3. States of Mental Depression—Melancholia (Psychalgia).

4. States of Mental Exaltation—Mania (Psychlampsia).

5. States of Alternation, Periodicity, and Relapse in Mental Disease (Folie Circulaire, Psychorhythm, Folie à Double Forme, Circular Insanity, Periodic Insanity, Recurrent Mania, Katatonia).

6. States of Fixed and Limited Delusion—Monomania (Monopsychosis).

7. States of Mental Enfeeblement (Dementia, Amentia, Psychoparesis, Congenital Imbecility, Idiocy).

8. States of Mental Stupor (Psychocoma).

9. States of Defective Mental Inhibition (Impulsive Insanity, Volitional Insanity, Uncontrollable Impulse, Psychokinesia, Hyperkinesia, Inhibitory Insanity, Insanity without Delusion, Exaltation or Enfeeblement, Affective Insanity); The Insane Diathesis.

10. General Paralysis, Paralytic Insanity (from gross brain disease).

11. Epileptic Insanity, Traumatic Insanity.
12. Syphilitic Insanity, Alcoholic Insanity.
13. Rheumatic and Choreic Insanities, Gouty and Podagrous Insanity, Phthisical Insanity.
14. Uterine or Amenorrhœal and Ovarian Insanity, Hysterical Insanity, Insanity of Masturbation.
15. Puerperal Insanity, Insanity of Lactation, Insanity of Pregnancy.
16. The Insanities of the Times of Life, Insanity of Puberty, Insanity of Adolescence.
17. Climacteric Insanity, Senile Insanity.
18. The Rarer and less Important Clinical Varieties of Mental Disturbances.
19. Medico-Legal and Medico-Social Duties of Medical Men in Relation to Insanity.

The author has not over-estimated the value of his work when viewed from the clinical standpoint. The descriptions of the diseases and cases are simple and practical, but true; and one sees as he reads that they are given by one perfectly familiar from daily observation with the cases and diseases he is speaking of. There is one feature of the book which commends it highly, and which is not to be found in any other work on mental diseases as far as I know, and it is the hints and descriptions given as to the practical management and care of the cases, very much as if he were in the hospital ward with you. There is nothing systematic about the arrangement of the subjects treated in each lecture ; this may be disappointing to some readers, but when it is considered that the lectures are intended for students before graduation, or for physicians who do not desire to have an extensive knowledge of this subject, the method of Dr. Clouston certainly commends itself, and in this respect as a class-book it is superior to the Psychological Medicine of Bucknill and Tuke. There are a few things in the work with which I cannot entirely agree ; I will point out a few of them.

In the chapter on melancholia the following forms are distinguished : Simple m., hypochondriacal m., delusional m., excited m., resistive (obstinate) m., epileptiform (convulsive) m., organic (coarse brain disease) m., suicidal or homicidal m. This appears to me an unnecessary division of the subject. Why should there be a form called " resistive " ? It is true we meet with cases who show a disposition to resist being dressed, moved, etc., but this does not appear sufficient reason. And why separate the suicidal into another form? And the epileptiform makes one suspect that this variety is an association of the melancholia of general paralysis with epileptiform attacks. This excessive division may be said to have the advantage of pointing out to the student a class of cases who, as Dr. Clouston says, resist persistently, doggedly, and unreasonably, and often with fierce violence, all that is done for them, and they are the most trying to the patience of the nurses. This, however, could all be pointed out in the clinical desciption, and we much prefer the division into melancholia simplex, attonita, and agitata.

Under the head of hypochondriacal melancholia two cases are described with this peculiar denomination: "Two cases of visceral melancholia (brothers); delusions that their bowels never moved, etc." This is confusing to a student who is trying to comprehend the nomenclature of mental diseases. There is no reason why a person who believes that his intestines are obstructed should be placed under a different class from the woman who says that one half of her throat is stopped up, and insists that every thing must be put on a certain side of her mouth or she cannot swallow it.

Good, plain descriptions are given of the periodic insanities, the monomanias, and degenerative states.

In the chapter on alcoholic insanity he says, after speaking of hallucinations in alcoholics: "I believe that more suicides and combined suicides and homicides result in the country from alcoholism in its early stages than from any other cause whatsoever."

This is undoubtedly the case in other countries, as well as Great Britain, and a large number of suicides which occur in asylums are probably among alcoholics; they are the most troublesome class to deal with in this respect.

Treatment is discussed at length and clearly when considering the diseases, and in connection with the relation of cases, and not placed in a separate chapter, as is usually done. Under the treatment of melancholia, after advocating tonics and nutritious diet, the question of medication is taken up, and quinine is placed in the first rank. Opium, he says, does no good, but decided harm by interfering with the appetite and digestion and diminishing the weight. Our experience in this respect differs from that of Dr. Clouston, with this difference, however, that he speaks of opium and our experience is with morphia. For years we have treated melancholia simplex and attonita by sulphate morphia with good results, and found that all our patients improved mentally and rapidly gained weight. In but one case have we had to discontinue its use, by reason of the constant vomiting which was produced. In all the cases we have treated, now quite a large number, we have not found any loss of appetite or interference with digestion ; constipation has been very slight, and only at the beginning of the treatment. The treatment by morphia has been advocated and carried out for a long time by Voisin and other European physicians.

Dr. Clouston advises in these melancholiacs at night bromide potass. and tinct. cannabis indica. This is not in accord with our practice. The entire treatment of the insane, medical and otherwise, is carefully, plainly, and satisfactorily pointed out.

In a short notice like this it is impossible to give quotations or an analysis of such a book ; it must be read, and we can heartily recommend it to the student and busy general practitioner.

[J. C. S.]

ADDENDUM.—Under the able editorship of Dr. Charles F. Folsom of Boston, Messrs. Henry C. Lea's Son & Co. have issued an American edition of this treatise—a well-printed volume of 550 pages. Dr. Folsom's work consists in an abstract of the lunacy laws and regulations in force in the various States. This matter is arranged alphabetically, and occupies about one hundred pages ; it greatly increases the value of Dr. Clouston's book for the American practitioner.—[EDITOR.]

Surgical Diagnosis. By AMBROSE L. RANNEY, A.M., M.D. Third edition. 8vo, pp. 608. New York : William Wood & Co., 1884.

The appearance of this edition, the third in five years, is evidence that the book has been found useful in professional work. The arrangement of the matter for ready reference, its fulness, and the "royal-road," seductive columns of contrasted symptoms have been its attractions. The present has been freed from many of the errors of the earlier editions. It has been enlarged by the addition of chapters on diagnosis and localization of diseases of the brain and spinal cord, which certainly should have afforded good reason for adding another word or two to the title. The book, improved by illustrations (probably drawn when artists were out of town) has many excellent traits to recommend it. [J. V. D.]

A Year-Book of Therapeutics for 1883. Edited by ROYAL W. AMIDON, M.D. G. P. Putnam's Sons.

For several years Dr. Amidon has been a successful lecturer on therapeutics,[1] and is therefore most appropriately situated for making such a useful review of the subject as this volume contains. Indeed, as one of the most important functions of a medical teacher is that of presenting to his pupils, each year, the actual state of science on the subjects of his department, the publication of an annual review might profitably be exacted of every one holding this responsible position. A very considerable amount of information is packed closely between the covers of Dr. Amidon's Year-Book. The proportion of space devoted to different subjects is, of course, not necessarily in accord with their intrinsic value, but with the degree of attention they have happened to receive from the year's regiment of investigators.

Dr. Rummo's monograph on iodoform, based on experiments performed in Vulpian's laboratory, is given entire. The translation is made by Dr. George B. Phelps. The monograph occupies forty-seven pages, and the total space devoted to the drug is fifty-four pages. Rummo's investigations, made in Vulpian's laboratory, are mainly physiological, and on phenomena which at present chiefly interest the therapeutist in cases of poisoning. Of the latter, by means of surgical dressings, Koenig has observed forty-eight cases, several fatal, with generalized fatty degeneration of the viscera. No amount (locally applied) under ten grammes proved dangerous.

The main therapeutic interest of iodoform at present attaches to its antiseptic properties. Rummo found that a solution of iodate of soda containing 5 : 1000 was necessary to prevent the development, but a solution ten times stronger was needed to kill germs already developed and actively propagating.

The antiseptic properties of the drug are the basis of an increasing number of therapeutic applications, not only in the dressing of wounds, but of scrofulous and tuberculous sores, in inhalation treatment of chronic pulmonary diseases, in fungoid synovitis, in inflammation (Binz.); possibly, also, in diabetes.

The next largest abstract in the Year-Book is on anæsthetics, which occupy twenty-one pages. Of these, thirteen are devoted to a discussion of the dangers of bichloride of methylene. Nine fatal cases, recorded in English medical journals, are discussed by Dr. Junker, and seven favorable cases of his own then related.

The third abstract of any length (twelve pages) sums up Sydney Ringer's experiments on the physiological action of chloride of barium; which he finds singularly similar to that of digitalis. No therapeutic use has yet been found for it. More vivid practical interest therefore attaches to corrosive sublimate, to which ten pages are given. Dr. Weir's paper is quoted, apparently at length. The only other long abstract is from a paper by Nancrede, read before the American Surgical Association, on the local processes of inflammation. This paper is interesting, if only because its suggestions compel the reader to go over again the endless topic of inflammation, and to peer once more into the depths of vital processes. Nevertheless it appears to us rather naïve. The author admits that the typical form of bleeding for arresting inflammation would be from the vein leading from the inflamed part. This is easy to do on a frog's tongue. But how may we do it in pneumonia, or peritonitis, or meningitis, or any other clinical inflammation, such as really tries men's souls?

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[1] At the Woman's Medical College of the N. Y. Infirmary.

Many other therapeutical topics are noticed, in concise but comprehensive abstracts of a paragraph or a page. Dr. Amidon, in a general review, alludes especially to the new treatments of phthisis which have been based on the bacillus theory of tuberculosis; to naphthalin as an antiseptic surgical dressing; to the newer treatments of malarial and typhoid fever, especially by resorcin, chinolin, carbamide, and kairine; to convallaria, whose utility seems to be overbalanced by its disadvantages; to the two new hypnotics, paraldehyde and aretal; and to the extensive experimentation on transfusion.

A complete alphabetical index lends additional value to this excellent compendium. [M. P. J.]

A Year-Book of Surgery for 1883. Edited by CHARLES H. KNIGHT, M.D. New York: G. P. Putnam's Sons, 1884.

This new enterprise in American medical writing comes in such good shape and so complete, that we give it hearty welcome and bespeak its permanency. The book, though having not quite two hundred pages, goes well over the ground of the year's work in surgery, and is divided into five parts, which include the surgery of the different regions of the body, general surgery, and venereal disease. The editor here presents his well-chosen cases concisely and intelligibly, with the instruction they were intended to convey. And, what is not a little gratifying, he has made frequent choice of cases from American work, which, although not dazzling, are nevertheless progressive and instructive. An introduction condenses into ten pages the more noteworthy lessons of the year, which are: impunity in operating through antisepsis; the place of, and the difficulties connected with, resection of the pylorus; thyroidectomy, and its teaching relative to myxœdema; the relative value of supra-pubic lithotomy; the value of digital exploration of the bladder through the perineum; the use of wire suture in transverse fractures of the patella; the relation of syphilis with rickets and locomotor ataxia; and the futility of excision of the primary syphilitic sore. [J. V .D.]

Veterinary Medicine and Surgery in Diseases and Injuries of the Horse. Compiled from standard and modern authorities and edited by F. O. KIRBY. Illustrated by four colored plates, and one hundred and sixty wood engravings. New York: William Wood & Co., 1883. Pp. 332.

This work forms the twelfth volume of "Wood's Library of Standard Medical Authors for 1883." The compiler states that it "has been prepared with a desire to present in a concise form a practical manual of the diseases and injuries of the horse and their treatment, for the use of practitioners of medicine and other intelligent horse-owners. The professional veterinarian already has at his command several large and excellent treatises. These, however, devote considerable space to the consideration of subjects familiar to the student of medicine and unnecessary to the non-professional reader, physiology and pathology are therefore seldom referred to in the present work."

It will be seen from the above quotation that the book has a twofold object. It is not only intended for the instruction of the medical profession, exclusive of veterinary practitioners, but is to serve also as a popular treatise. Its aim is even a more ambitious one than that attempted in works entitled "The Family Physician," whose authors are satisfied with catching the popular eye without expecting that they will assume the pretensions of text-books for the medical profession also. Perhaps the compiler, who, we infer, is a non-professional writer, from the absence of a title and the fact that the only claim that he makes to practical medical knowledge is the result of sixteen years' experience in the *ownership* and *consequent* care of horses, is right in classing the medical profession along with the "other intelligent horse-owners," and in separating both classes from "the professional veterinarian"; for, as far as the appreciation of the close relationship which exists between the pathology of the lower animals and that pertaining to man are concerned, it cannot be accredited to the average physician to a very high degree; nor is this state of affairs likely to be improved by the work before us; in which, on the author's own admission, physiology and pathology are seldom referred to. In reading this book the horse-man will have the advantage over the doctor of medicine, for the nomenclature is in the vernacular of the men who "talk horse." He will revel in the terms belly-ache, blind-staggers, warbles, sit-fasts and rat, tails, scratches and seedy-toe; though the physician may occasionally find relief in a synonym which exhibits an attempt to truly represent a pathological condition in the terms of general pathology.

That the compiler has attained the object set forth in his preface quoted above we do not deny. He has produced a work, however, which must of necessity be a superficial one, and which might well be called "Every Man (Physicians Included) his own Horse-Doctor." Such productions are far from elevating in their effect on veterinary medicine. Works on hygiene, and the application in diseased states, of simple measures, which cannot do harm, with indications as to when to call for professional aid, is all that a popular work on either human or veterinary medicine can with safety attempt.

The details of treatment, and posological statements, while they may be of service to the physician, are as dangerous in the hands of the non-medical horse-owner as our works of materia medica would be in the hands of a layman. Why such a volume should have been added to a "Library of Standard Medical Authors" it is difficult to understand, unless it be a sop thrown to the country practitioner, who is often called upon to apply his medical knowledge to the treatment of disease in the lower animals. To such we would say, that they will do better to rely on their general knowledge of the principles of pathology, than to trust to a brief *résumé* of diseases of the horse, as such compilation must be, however well done; or else consult some comprehensive work on veterinary medicine, such as William's, Robinson's and others. Let the physician, as well as the layman, remember, however, that veterinary medicine is a specialty by itself, and requires as great skill as any other department in the field of medicine. Valuable animals ought to be intrusted to the care of those who are specially skilled in this practice, rather than to the layman with his dangerous smattering, or even the physician, with his general medical knowledge. The profession and the public should foster the methods of education that fit men for veterinary practice, which is just lifting itself up out of the chaos of quackery.

On the other hand, the scientific student of medicine will do well to devote more time to the investigation of the diseases of lower animals, and not hold himself aloof from his veterinary brother, from whose field of work he may obtain a wealth of knowledge indispensable to the future advancement of scientific medicine. Medical science is not limited to the diseases of man ; it embraces diseases of all organisms, and there is no branch of it which has not its important relations to every other branch. It is comparative medicine which will rank as scientific medicine of the future, but human medicine will be the practical gainer thereby. It will not be accomplished, however, by such literature as we find in the volume before us, which, although it may inspire a few to look deeper into the subject, and furnishes many valuable and practical hints, will rather serve to bolster up the pretenders to veterinary medicine. [W. R. B.]

A Manual of Obstetrics. By A. F. N. KING, M.D. With fifty-nine illustrations. Second edition. Henry C. Lea's Son & Co., Philadelphia, 1884, pp. 338.

The writer of this manual does not claim originality for his book. He has produced a compendium, or abstract, of Playfair, Lusk, and Leishman, to save his pupils the trouble. That the latter appreciate what he has done for them is evident since a second edition is issued by its publishers.

It is doubtful that the hopes of its author will be realized that " those whose onerous duties allow but little leisure for consulting larger works " will find value in its pages. Such practitioners generally have learned the simples of obstetric practice, if not from their college professors words of wisdom, from that best of teachers, Experience, and will only look for instruction in the most elaborate works, or the papers on special subjects found in current literature.

A valuable addition to the obstetric manual is the chapter on " The Jurisprudence of Midwifery," which is based on the works of Dr. Alfred Swaine Taylor and Dr. Beck. It is here that the professional man, "laden with onerous duties," might find help, since his appearance in court is not an every-day affair, and undoubtedly he could make a better show on the witness-stand if he should have the little book to read on his way thither. [G. P.]

Compendium der pathologisch-anatomischen Diagnostik. Von Dr. JOHANNES ORTH, Prof. der allgemeinen

Pathologie und pathologischen Anatomie in Göttingen. Dritte Auflage, pp. 634. August Hirschwald, Berlin, 1884.

Prof. Orth has greatly added to this edition of his work. Sections on microscopic technique, on the examination of sputa, fæces, urine, etc., are new. The first is especially valuable as containing a summary of all that is worth knowing relative to the recognition and study of bacteria and other living disease germs. As the author himself says—these additions furnish the elements of so-called clinical microscopy.

As is logical in a work intended for use chiefly at the post-mortem table, the subjects are arranged upon the basis of topographical anatomy ; cavities, apparatuses, and organs being studied *in situ*, and then detached in the course of the systematic post-mortem. Under the head of each organ, however, follows a study of its alterations in general and in certain diseases, so that to a certain extent the work is also a treatise on pathological anatomy.

The introduction, occupying twenty-five pages, consists of general rules for making an autopsy, an enumeration of the necessary instruments, and a detailed account of microscopic and micro-chemical manipulations.

The subject-matter is broadly divided into two great parts—one treating of the examination of the external parts of the body, and the other that of its interior. Under the first subdivision we find descriptions of the attitudes and physical attributes of the corpse, a minute account of the morbid alterations affecting the skin and its epithelial appendages. One section is devoted to the condition of the new-born child. This first part occupies thirty-eight pages.

The rest of the treatise is occupied by the second part of the subject, the study of the inner parts of the body, arranged in four sections : 1. The spinal cavity and contents. 2. The cranial cavity and contents, including the eye and ear (in sixty-four pages). 3. The thoracic and abdominal cavities and their contents. 4. The extremities (including lymphatic glands, blood-vessels, nerves, muscles, joints, and bones). Particularly interesting are the sections on diseases of the cerebral membranes, on the blood, on the lungs, on sputa, on the kidneys, on the stomach, on the intestines, and on the muscles.

Orth recognizes the following forms of nephritis : (*a*) suppurative, (*b*) degenerative parenchymatous—acute and chronic (latter with extensive fatty degeneration), (*c*) glomerulo-nephritis, (*d*) granulating or acute productive interstitial, with varieties : (d^1)

interstitial glomerulo-capsular (cystic formation and shrinkage), (d^2) multiple chronic fibrous interstitial (syphilitic), (d^3) interstitial papillary nephritis.

The author has firm faith in the existence and significance of the bacillus Kochii : he has demonstrated its presence in sputa, in various forms of tubercular formations in the lungs, in the fine tubercles of the cerebral and spinal membranes, in the intestines, articulations, etc.

In connection with different organs the various parasites are thoroughly described.

This work is truly admirable, and will be enjoyed more especially by the working pathologist, though the advanced student and practitioners will find in it an enormous mass of facts clearly stated and grouped in such a way as to make their comprehension and retention easy. We earnestly hope that an American edition will be issued ere long. [E. C. S.]

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SURGERY, AND THEIR SPECIAL BRANCHES

EDITED BY
E. C. SEGUIN, M.D.

> S'il est possible de perfectionner l'espèce humaine, c'est dans la médecine qu'il faut en chercher les moyens.
>
> —DESCARTES

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ARCHIVES OF MEDICINE.

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(*Concluded.*)

THE THERMIC PHENOMENA IN CONTRACTION OF MAMMALIAN MUSCLES.

(Second communication from the Leipzig Physiological Institute.[1])

BY ROBT. MEADE SMITH, M.D.,

PROFESSOR OF COMPARATIVE PHYSIOLOGY IN THE UNIVERSITY OF PENNSYLVANIA.

IN my first communication on the heat development of contracting muscle, I showed that in a long tetanus from maximal irritation, the muscle still in connection with the circulation at first rapidly increased in temperature until much warmer than the arterial blood; that the temperature then slowly increased until a maximum was reached, when, from the decreasing muscle temperature and the increasing arterial temperature, the temperatures of muscle and blood even during the tetanus commenced gradually to approach one another. No conclusion, however, was then drawn as to whether the fall of temperature toward the end of the tetanus was attributable to a more rapid loss of heat through the increase in the blood supply, or to a diminished heat production. The method which I have described above permits the alternate arresting and restoring of the blood supply, during the course of a tetanus of several minutes' duration, and therefore, by the variation of the conditions of the experiment, enables the formation of a conclusion as to this point. For if the cessation of increase of temperature during a tetanus is due to the cooling influence of the blood, arrest of circulation should cause the temperature which had become constant, or even commenced to fall, to again commence to rise; if, however, under these circumstances the temperature does not increase, it must be concluded that heat production in the muscle has been interfered with, the degree being capable of recognition by the comparison of the temperature of the bloodless muscle during and after irritation.

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[1] See du Bois Raymond's *Archiv für Anatomie und Physiologie*, Physiologische Abtheilung, 1881, p. 105.

In the following experiments of this nature, animals were selected which were especially capable of energetic heat production. In the first experiment the circulation was not disturbed during the irritation until the temperature of the muscle ceased to rise; in the second, the aorta was closed before the muscle temperature reached its maximum. In the first column of figures to the left is noted the temperature of the muscle immediately before the commencement of the irritation, the succession of figures to the right giving the temperature of the muscle during contraction at intervals of one minute. From the slightness of its changes, the thermometer readings of the arterial temperature are only given at the beginning and end of each period of the experiment; of these four periods, the first three represent contraction, the fourth rest.

The following curve (fig. 1) has been constructed from the figures of the preceding table. The upper heavy curve represents the temperature-changes in the muscle; the lower lighter curve, the temperature of the blood. The intervals on the abscissa line correspond to minutes; the heavy ordinates represent the blood-pressure in the femoral artery in millimetres of mercury, while the dotted perpendicular lines represent the duration of irritation and of closure of the aorta.

It is to be noticed in the above experiment that before

I.

	Time after Commencement of Irritation, in Minutes.															Total Change in Temperature.	
1. Aorta open.		1	2	3	4	5											Tetanus.
Muscle temperature .	37.83	38.39	38.55	38.70	38.76	38.80° C.										+0.97°	
Temperature increase		0.56	0.16	0.15	0.06	0.04° C.											
Temperature of blood	37.70					37.75° C.											
	Time after Commencement of Irritation, in Minutes.															Total Change in Temperature.	
2. Aorta closed.	6	7	8	9													
Muscle temperature .	38.80	38.80	38.80	38.80												0°	
Temperature increase	0.0	0.0	0.0	0.0													
	Time after Commencement of Irritation, in Minutes.															Total Change in Temperature.	
3. Aorta opened.	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24		
Muscle temperature .	38.80	38.55	38.45	38.30	38.18	38.10	38.08	38.04	38.00	38.00	38.00	38.00	38.00	38.00	38.00	—0.80°	
Temperature decrease		—0.25	—0.10	—0.15	—0.12	—0.08	—0.02	—0.04	—0.04	0.0	0.0	0.0	0.0	0.0	0.0		
Temperature of blood	37.70	37.75	37.72											37.70			
	Time after End of Irritation, in Minutes.															Total Change in Temperature.	
4. Aorta open. — No irritation.		1	2	3	4												Rest.
Muscle temperature .	38.0	37.95	37.94	37.92	37.87											—0.13°	
Temperature decrease		—0.05	—0.01	—0.02	—0.05												
Temperature of blood	37.70				37.70												

the commencement of irritation the temperature of the muscle was 0.13° C. warmer than the arterial blood and that in the first four minutes of contraction this difference was increased to 1.05° C. As the temperature of the muscle then remained constant for thirty seconds, in the presence of the circulation, it was to be expected that if the circulation was arrested, and so one source of cooling removed, the temperature of the muscle would again ascend. Such was not, however, the case: the muscle temperature remained constant during the four minutes that the aorta was closed, and then rapidly sank, when the blood current was restored, to within 0.28° C. of its original temperature, where it remained unaltered until the end of the contraction and then slowly sank to its original stand.

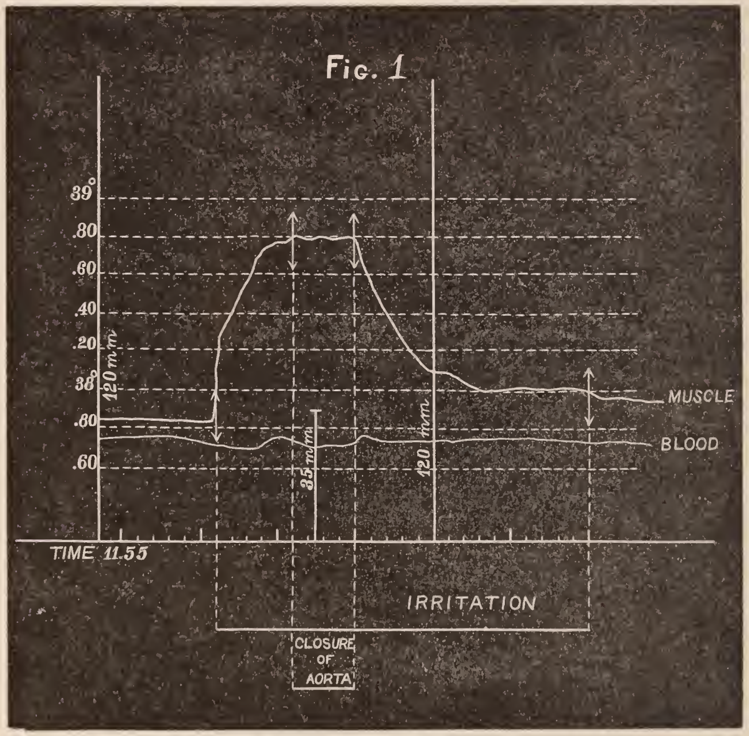

Fig. 1.

II.

	Time after Commencement of Irritation, in Minutes.										Total Change in Temperature.	
1. Aorta open.		1	2	2½								Tetanus
Muscle temperature	38.37	38.85	39.07	39.23° C.								
Temperature increase		0.48	0.22	0.16° C.							+ 0.86° C.	
Temperature of blood	38.08			38.08° C.								
	Time after Commencement of Irritation, in Minutes.										Total Change in Temperature.	
2. Aorta closed.	3	4	5	6	7							
Muscle temperature	39.23	39.26	39.27	39.28	39.30							
Temperature increase		0.03	0.01	0 01	0.02						+ 0.07°	
	Time after Commencement of Irritation, in Minutes.										Total Change in Temperature.	
3. Aorta opened.	8	9	10	11	12	13	14					
Muscle temperature	39.30	39.15	38.84	38.83	38.83	38.77	38.74					
Temperature decrease		— 0.15	— 0.31	— 0.01	0.0	— 0.06	— 0.03				— 0.56°	
Temperature of blood	37.95	38.21					38.17					
	Time after End of Irritation, in Minutes.										Total Change in Temperature.	
4. Aorta open.—No irritation.		1	2	3	4	5	6	7	8	9		Rest
Muscle temperature	38.74	38.56	38.45	38.36	38.29	38.29	38.29	38.29	38.29	38.29		
Temperature decrease		— 0.18	— 0.11	— 0.09	— 0.07	0.0	0.0	0.0	0.0	0.0	— 0.45	
Temperature of blood	38.21			38.18			38.15					

The same explanations apply to the following curve, constructed from the above table, as were given for fig. 1.

The above experiment differs from No. I. of the same series in that in this instance the aorta was closed two and a half minutes, instead of five minutes, after the commencement of the irritation, and consequently before the maximum temperature was reached by the muscle; it was therefore to be expected that the temperature would continue to rise after the arrest of the circulation, and it was found that while the muscle increased from 0.29° C. above the temperature of the blood to 1.15° C. in the first period of the experiment, there was a still further gradual addition during the first four minutes, when the aorta was closed. When, however, the circulation was reopened, the muscle temperature fell in two minutes to within 0.63° C. of the

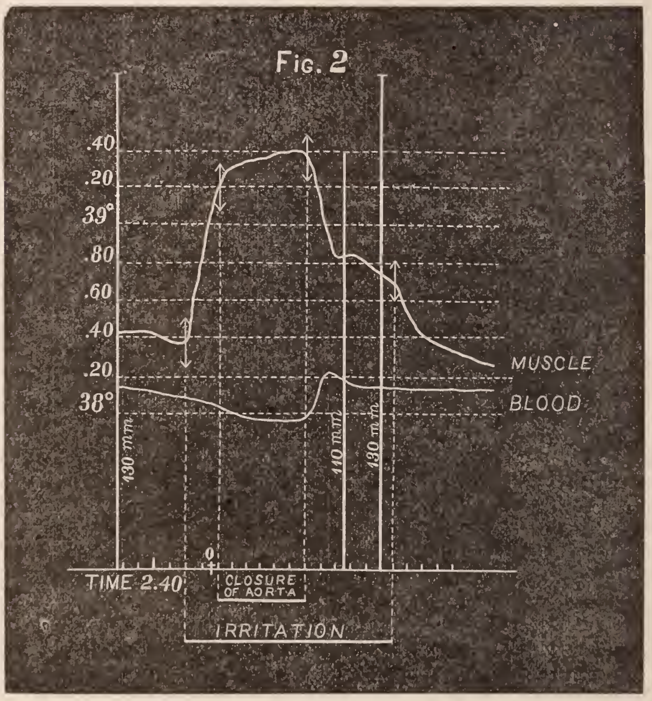

FIG. 2, *a*.

blood temperature, remained nearly constant at this point for two minutes, and then gradually sank still farther during the remainder of the tetanus. When the irritation ceased, the muscle temperature then sank to within 0.14° C. of that of the blood, or 0.15° C. lower than its original position.

By examination of fig. 1 it will be seen that the blood-pressure in the femoral arteries, which at the commencement of the closure of the aorta stood at 0, rose to 30 mm. mercury before the aorta was reopened, from the causes already mentioned in the commencement of this communication as explaining that occurrence. But from the fact that the general course of the experiment corresponds very closely with that of the second experiment, where the closure of the aorta remained complete, it must be concluded that the current in the first case was too feeble to influence the thermic conditions of the muscle.

It may therefore be concluded that the temperature-changes of a muscle in connection with the circulation and contracting under the influence of maximal irritation are dependent mainly upon changes in the heat production of that muscle; and that the heat production which is most intense at the commencement of contractions, gradually decreases in energy until almost entirely suspended. Another fact in favor of this explanation is that during the period in which the temperature-increase commences to decline, the circulation through the muscle remains nearly constant; therefore, since the cessation of increase of temperature cannot be attributed to an increase in loss of heat through an increased circulation, it must be due to diminished heat production. And since we have found that after the arrest of circulation there is no further increase in the temperature of the muscle, diminished heat-production must also be concerned in the fall of temperature which occurs toward the end of prolonged tetani, even when the circulation has not been interfered with.

In order to form a conclusion as to whether the presence of the arterial blood increases the capability of developing heat, the temperature-changes of the normal and bloodless muscles, thrown into contraction by maximal irritation, must be compared. From what has already been stated, it is evident that an excess of heat production in the normal muscle[1] over that produced by the muscle removed from its blood-supply, can only be admitted when the degree and duration of temperature-increase in the first, under unfavorable conditions, largely exceed those developed under more favorable conditions in the latter. Accordingly, from the fact that the results of fatigue are not completely removable by short periods of repose, in the comparison of the degrees of heat production of the bloodless and normal muscle the contraction of the bloodless muscle must precede that of the muscle still supplied with blood. If under such conditions the normal muscle acquires a higher temperature and maintains it longer than the bloodless muscle, the presence of the circulation must aid the development of a larger amount of heat, for the muscle still within the circulation is more fatigued and subject to greater loss of heat than the bloodless muscle.

Of the facts brought out in my experiments made after the above plan, I will first give the comparisons of the maximum temperature-increase produced by irritation of the bloodless and normal muscle.

No. I.—1st. Tetanus. *Without circulation.* Temperature increased from 39.25° to 39.63° C. Maximum = 0.38° C.

2d. Tetanus. *With circulation.* Temperature increased from 39.46° to 39.96° C. Maximum = 0.50° C. Muscle temperature 0.49° C. over blood.

3d. Tetanus. *Without circulation.* Temperature increased from 39.65° to 39.95° C. Maximum = 0.30° C.

4th. Tetanus. *With circulation.* Temperature increased from

---

[1] This term will be repeatedly employed to designate a muscle whose circulation has not been interfered with.

39.75° to 40.12° C. Maximum = 0.37° C. Muscle temperature 0.40° C. over blood.

No. II.—1st Tetanus. *Without circulation.* Temperature increased from 34.59° to 35.11° C. Maximum = 0.52° C.

2d. Tetanus. *With circulation.* Temperature increased from 34.59° to 35.27° C. Maximum = 0.68° C. Muscle temperature 0.80° C. over blood.

No. III.—1st. Tetanus. *Without circulation.* Temperature increased from 38.27° to 38.68° C. Maximum = 0.41° C.

2d. Tetanus. *With circulation.* Temperature increased from 38.08° to 38.84° C. Maximum = 0.76° C. Muscle temperature 0.83° C. over blood.

No. IV.—1st. Tetanus. *Without circulation.* Temperature increased from 32.75° to 33.13° C. Maximum = 0.38° C.

2d. Tetanus. *With circulation.* Temperature increased from 32.10° to 32.54° C. Maximum = 0.44° C. Muscle temperature 1.07° C. over blood.

No. V.—1st. Tetanus. *Without circulation.* Temperature increased from 39.26° to 39.72° C. Maximum = 0.46° C.

2d. Tetanus. *With circulation.* Temperature increased from 39.10° to 39.50° C. Maximum = 0.40° C. Muscle temperature 0.32° C. over blood.

Taking an average from the above figures, we find that for the fresh bloodless muscle, protected from loss of heat through the blood, the maximum increase of temperature as a mean was 0.41° C., while for the muscle which entered contraction while already in a condition of fatigue, and through which blood was circulating, which, as an average, was 0.65° cooler than the temperature reached by the muscle, the average maximum increase of temperature was 0.53° C. The share of the blood in the heat development by the muscles is still further emphasized when it is noticed that, with a single exception, in all the above experiments the increase of temperature was much greater in the case of the fatigued muscles which still had their normal blood supply, than in the fresh bloodless muscles; and in the one exceptional case the temperature of the fresh bloodless muscle was only 0.06° C. higher than the more fatigued muscle which still had its blood supply.

In order to form any idea as to the manner in which the blood influences the temperature-changes in contracting muscle, a distinction must be drawn between the characters of the circulation in different periods of contraction. For, at the onset of contraction the circulation through the muscle, as Gaskell has shown,[1] is slackened to such an extent that the conditions are very much the same in the muscles with normal blood supply, and in those whose circulation has been arrested by closure of the aorta, rendering it probable that in the early stages of contraction the development of heat is equal in both instances; since even in the normal muscle the heat development occurs so rapidly that the changes occurring in the interior of the muscle are at first uninfluenced by the sluggish blood-stream. The following table gives the result of my experiments on the influence of the blood current on heat production in the later stages of contraction. The first column of temperature-figures to the left gives the temperature of the muscle immediately before the commencement of contraction, the following figures giving the increase in temperature at every subsequent thirty seconds.

From the commencement of the tetanic irritation until the thirtieth second the temperature increases in the normal and bloodless muscles with nearly equal rapidity; in the subsequent periods of the tetanus, however, we find a marked difference. With the exception of a single case we find that in the bloodless muscle the maximum temperature-increase is always attained within one and a half to three minutes, while in the muscles still within the circulation the temperature continues to increase for two and a half to four and a half minutes. From the fact, therefore, that the normal contracting muscle is able to produce heat for a longer period than the bloodless muscle, it is evident that the blood enables the normal muscle to prolong the heat-

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[1] "Arbeiten des Leipziger physiolog. Instituts," 1877.

Time after commenc'm'nt of contract'n.		30"	1 min.	1½ min.	2 min.	2½ min.	3 min.	3½ min.	4 min.	4½ min.	5 min.	5½ min.	6 min.	6½ min.	
I.															
1st. Tetanus	39.25°	+ 0.22	+ 0.14	+ 0.02	0.00										Without circulation.
2d. Tetanus	39.46°	+ 0.27	+ 0.14	+ 0.08	+ 0.02	+ 0.0									With circulation.
3d. Tetanus	39.65°	+ 0.22	+ 0.06	+ 0.02	0.00										Without circulation.
II.															
1st. Tetanus	34.59°	+ 0.14	?	+ 0.05	+ 0.04	+ 0.04	+ 0.06	+ 0.02	+ 0.03	+ 0.04	?	+ 0.05	?	+ 0.05	Without circulation.
2d. Tetanus	34.39°	+ 0.25	+ 0.11	+ 0.12	+ 0.08	+ 0.06	+ 0.04	+ 0.01	0.						With circulation.
3d. Tetanus	34.50°	+ 0.20	+ 0.08	+ 0.02	+ 0.02	0.0									Without circulation.
III.															
1st. Tetanus	37.83°	+ 0.46	+ 0.10	+ 0.06	+ 0.10	+ 0.06	+ 0.09	+ 0.05	+ 0.01	+ 0.03	0.0				With circulation.
2d. Tetanus	38.27°	+ 0.24	+ 0.06	+ 0.02	+ 0.01	+ 0.03									Without circulation.
3d. Tetanus	38.08°	+ 0.22	+ 0.24	+ 0.16	+ 0.08	+ 0.05	+ 0.01	0.0							With circulation.
4th. Tetanus	38.27°	+ 0.20	+ 0.10	+ 0.05	+ 0.03										Without circulation.
IV.															
1st. Tetanus	39.38°	?	+ 0.32	+ 0.18	+ 0.02	+ 0.11	+ 0.06	+ 0.03	0.0						With circulation.
2d. Tetanus	39.64°	+ 0.23	+ 0.04	+ 0.02	+ 0.01	+ 0.01	+ 0.03	0.0							Without circulation.
3d. Tetanus	39.27°	+ 0.28	+ 0.09	+ 0.13	+ 0.14	+ 0.09	+ 0.07	+ 0.03	+ 0.01	0.0					With circulation.

producing changes which at first are of equal intensity in both bloodless and normal muscles.

We see, therefore, that the blood not only in the periods of rest, but also during tetanic contraction, modifies the thermic phenomena in muscles.

That the above rule is not without exceptions is seen in the first tetanus of No. II. of the above series. There the temperature in the bloodless muscle continued to increase for six and a half minutes, though this tetanus differed from the other tetani in the same animal and in the others of the same series by the extreme slowness with which the maximum temperature was attained; since in the six and a half minutes during which the increase in temperature continued the maximum reached was only 0.52° C. as contrasted with an increase of 0.62° C. attained in the following tetanus of only three and a half minutes, when the muscles were still within the circulation.

If the changes leading to a development of heat in the contraction of bloodless muscles are hindered by the absence of oxygenated blood, it is conceivable that the reopening of the circulation during a tetanus of bloodless muscles after the increase of temperature had ceased might again furnish the necessary conditions for the further development of heat in such a degree as to cause a second increase of temperature. Such a state of affairs occurred in several of my experiments, as seen in the following table.

		Muscle Temperature.	Temperature-Increase in each Minute.	Total Increase.
I.	Aorta closed.	34.34° C.	+0.18, +0.02, +0.01, 0.00,	+0.21° C.
	Aorta opened.		+0.05, +0.08, +0.04, +0.04, 0.04,	+0.23° C.
	Muscle temperature +0.68° C. over blood.			
II.	Aorta closed.	38.27° C.	+0.30, +0.08, +0.0,	+0.38° C.
	Aorta opened.		+0.07, +0.13, +0.08, +0.03,	+0.31° C.
	Muscle temperature +0.77° C. over blood.			
III.	Aorta closed.	36.21° C.	+0.24, +0.0,	+0.24° C.
	Aorta opened.		+0.08, +0.13, +0.03, +0.01, 0.00,	+0.25° C.
	Muscle temperature +0.70° C. over blood.			

The following curve, constructed from the figures of the third tetani of the above series, serves to make the results of the above experiments more evident; the same explanations apply as in the case of fig. 1.

The above experiments, which are confirmed by several others, greatly assist in the explanation of the share of the blood in the internal changes in contracting muscles. For the chemical processes, which in contracting bloodless muscles have sufficient energy to develop enough heat to maintain the muscle temperature from 0.2° to 0.4° above that of the surrounding media, are so increased in energy with the restoration of the circulation that the muscles still further increase in temperature; in spite of the loss of heat attendant upon the circulation of a blood current which may be from 0.68° to 0.77° C. cooler than the muscle.

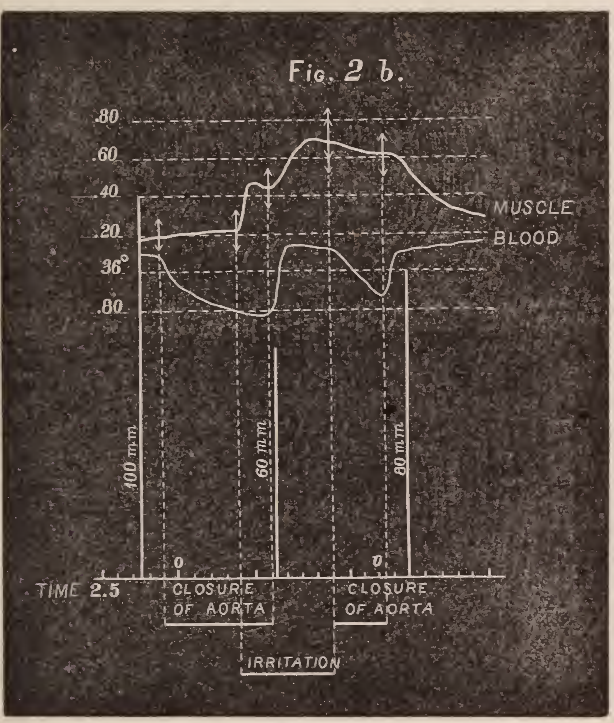

FIG. 2, *b*.

Although I have notes of several other experiments which give the same general results as detailed above, the reopening of the circulation in bloodless muscles during the course of a contraction does not invariably produce such an increase of temperature as in the examples given above. Sometimes the temperature only increases a few hundredths of a degree, and often the cooling influence preponderates over the assistance to heat production; the latter is almost invariably the case when the contraction has lasted more than six minutes before the circulation is reopened.

In the cases which have as yet been considered the muscles which were subjected to irritation, after being deprived of their blood supply, were, during the greater part of the long intervals of repose which followed each contraction, again receiving their normal blood supply. And, although at the end of the contraction the temperature of such muscle was not as greatly increased as after contraction with normal blood supply, the muscle temperature of the bloodless muscle was, nevertheless, considerably above its original temperature and that of its surrounding media.

The following modifications of the plan of procedure were made to permit of the study of the influence which the blood-current exerts on resting muscle after being fatigued and warmed by previous contraction.

The muscles, either with or without their normal blood supply, were first subjected to prolonged irritation; coincident with the cessation of irritation, the aorta was closed (if the circulation had not been already arrested, or was not restored if already closed) and the muscles, deprived of their blood-supply, allowed to rest for ten or fifteen minutes, and then again thrown into contraction without renewing the circulation. Therefore, the muscles were compelled to recover from their fatigue and again contract without receiving any assistance from the arterial blood. These conditions, therefore, reduced the mammalian muscles to the state which alone exists in excised frog's muscles.

Experiments made on the above plan, showed that the muscles, after irritation and an interval of rest while removed from the circulation, were again able to develop heat when again thrown into contraction, but to a considerably less degree than when, after contracting in the bloodless condition, they were a second time thrown into tetanus after an interval of rest in which the circulation was present.

The following experiments show that in the resting muscle contact with the arterial blood facilitates the development of heat in contraction.

I.	Initial Temperature.	Maximum Increase.
1*st* *Tetanus.*—The muscles had rested for half an hour with normal blood supply. The aorta was closed at commencement of irritation .	39.26° C.	0.46° C.
2*d* *Tetanus.*—The aorta was kept closed during an interval of rest of ten minutes after the first tetanus and during the second tetanus .	39.46° C.	0.08° C.
3*d* *Tetanus.*—The aorta was opened after the end of the second tetanus, and the circulation was undisturbed during a pause of ten minutes after the second tetanus, and during the third tetanus	39.10° C.	0.40° C.
Muscle temperature + 0.30° C. over blood.		

The course of the above experiment is reproduced in the following curve (fig. 2, *c*):

II.	Initial Temperature.	Maximum Increase.
1*st* *Tetanus.*—Duration = 7 minutes. During the tetanus and during a pause of ten minutes before the tetanus the aorta was open .	39.44° C.	+ 1.00° C.
Muscle temperature + 1.00° C. over blood.		
2*d* *Tetanus.*—Duration = 5 minutes. The aorta was closed immediately after the end of first tetanus, and kept closed during a pause of fifteen [1] minutes, and during second tetanus	39.97° C.	+ 0.23° C.
3*d* *Tetanus.*—Duration = 5 minutes. The aorta was opened immediately after end of		

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[1] During the long closure of the aorta, the circulation was not all the time completely arrested; the pressure in the crural artery, however, during this time, never rose above 10 mm. Hg.

second tetanus and kept open during the subsequent pause of fifteen minutes, and during the third tetanus . . . . . 39.48° C. + 0.74° C.

Muscle temperature + 0.75° C. over blood.

4*th* *Tetanus.*[1]—Duration = 6 minutes. The aorta was closed immediately after the end of the third tetanus and kept closed during the subsequent pause of twenty minutes, and during the fourth tetanus . . . . . 39.87° C. + 0.34° C.

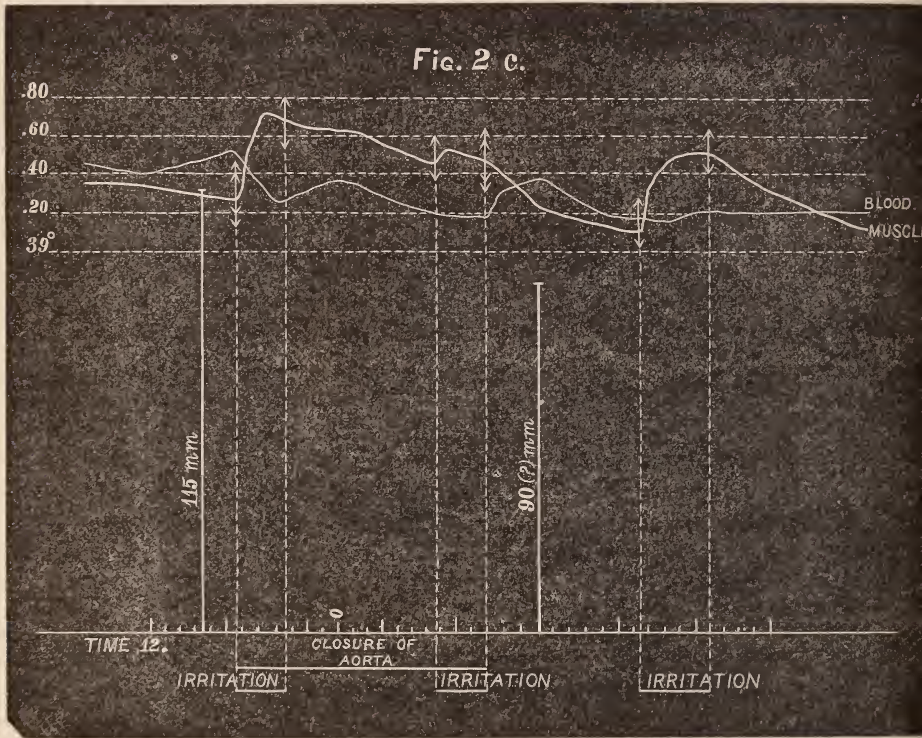

FIG. 2, *c.*

5*th* *Tetanus.*—Duration = 9 minutes. Immediately after the end of the fourth tetanus the aorta was opened and kept open during the subsequent pause of twenty minutes, and during the fifth tetanus . . . . . 39.33° C. + 0.85° C.

Muscle temperature + 0.60° C. over blood.

III.

1*st* *Tetanus.*—Duration = 5 minutes. Aorta open during the course of this tetanus and for the preceding ten minutes' rest . . . . 38.35° C. + 0.80° C. (?)

Muscle temperature + 0.60° C. over blood.

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[1] Up to this time sub-maximal irritation had been employed ; the strength of the current was now increased up to a maximal irritation.

2*d Tetanus.*—Duration = 5 minutes. Aorta was closed immediately after end of first tetanus and kept closed during the subsequent pause of ten minutes, and during second tetanus .	38.71° C.	+ 0.1° C.
3*d Tetanus.*—Duration = 5 minutes. The aorta was opened immediately after the end of the second tetanus and kept open during the subsequent pause of ten minutes, and during the third tetanus	38.22° C.	+ 0.53° C.

Muscle temperature + 0.53° C. over blood.

It is seen from the above that the resting bloodless muscle can recover from its inability of developing heat, but that this recovery is much more imperfect than when the circulation is present, and that the rapid initial rise of temperature is also in such cases absent. The excess of heat production, therefore, in a muscle which is supplied with blood during its period of rest, and deprived of it during its contraction, over that of the muscle removed from the circulation in both these periods, proves that the arterial blood plays an important part in the conditions essential to heat production by muscles. For were the amount of heat dependent on the store of combustible matter in the muscle, it is clear that the muscle removed from the circulation must liberate less heat than one whose blood-supply is undisturbed.

If fatigued muscles, in the absence of their blood supply, only slightly recover their capacity for heat production, and if, on the other hand, the normal muscle develops more heat in its contraction than the bloodless muscle, the assumption is warrantable that by reversing the conditions of the experiment the course of the temperature-changes in muscle may be radically changed. Therefore, if a fatigued muscle is deprived of its blood supply during its period of rest, and the circulation restored at the moment of commencement of a second irritation, if the above views are correct, the initial increase of temperature will be much less than in the normal muscle, but it will be more gradual and prolonged. The following experiment was performed to test the truth of these arguments:

	TIME (Minutes).											Total Increase.
1st Tetanus . . . . . The circulation was undisturbed before and during the irritation.	½	1	1½	2	2½	3	3½	4	4½	5	5½	
Muscle temperature, 37.94° .	38.17	38.26	38.35	38.45	38.54	38.61	38.67	38.71	38.73	38.76	38.77	
Increase in each 30" . .	0.23	0.09	0.09	0.10	0.09	0.07	0.06	0.04	0.02	0.03	0.01	+0.83° C.

The difference in temperature of the muscle over that of the blood increased from —0.01° to +0.80° C.

2d Tetanus . . . . . The circulation was arrested immediately after end of first tetanus, and aorta kept closed during fifteen minutes' rest, and then opened at commencement of second tetanus.	½	1	1½	2	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	
Muscle temperature, 38.17° .	38.16	38.16	38.19	38.21	38.24	38.27	38.31	38.37	38.42	38.49	38.53	38.58	38.61	38.63	38.63	38.63	
Increase in each 30" . .	0.01	0.0	0.03	0.02	0.03	0.03	0.04	0.06	0.05	0.07	0.04	0.05	0.03	0.02	0.0	0.0	+0.46° C.

The difference in temperature of the muscle over that of the blood increased from +0.17° to +0.70° C.

3d Tetanus . . . . . The circulation was undisturbed during a pause of fifteen minutes after the second tetanus and during the third tetanus.	½	1	1½	2	2½	3	3½	4	4½	
Muscle temperature, 38.08° .	38.18	38.28	38.46	38.51	38.54	38.58	38.59	38.60	38.60	
Increase in each 30" . .	0.10	0.10	0.18	0.05	0.03	0.04	0.01	0.01	0.0	+0.52° C.

The difference in temperature of the muscle over that of the blood increased from +0.13° to +0.54° C.

The following curve was constructed from the first and second tetani of the above experiment, in order to render the contrast between the rate of increase of temperature more evident.

It is thus seen that the arterial blood plays an important part both in the formation of the heat-giving substances in resting muscle, and in their combustion in contraction; and that even if, although not yet proven, the difference between the bloodless muscle and the muscle still in connection with the circulation is simply a quantitative one, it is nevertheless too important to be neglected. The restriction, therefore, of study to the bloodless muscle alone would be open to the same objection as the application of conclusions drawn from the study of the tissue-changes during hunger to the general metabolic processes of the body. Perhaps in the future it may be possible to isolate the sub-

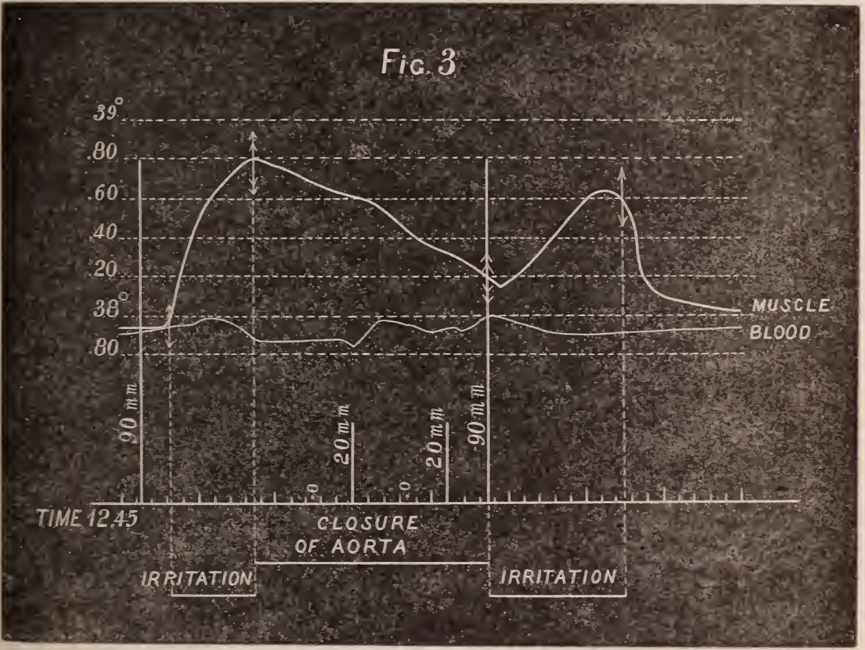

FIG. 3.

stances in muscle through whose combustion heat is developed; but even should this most difficult problem in the chemistry of muscle be solved, measurements of temperature-changes during contraction in the normal muscle, as to their duration and character from the commencement of contraction to the highest degree of fatigue, must still furnish an indispensable and accurate means of studying the course of the process of combustion of those bodies. And, finally, it is only by the study of the temperature-changes in the resting and contracting muscle while still within the circulation, that any idea can be formed as to the share which the muscles play in maintaining the general body temperature; while the comparison of the behavior of the normal and bloodless muscle in this connection is alone sufficient to render indispensable investigations similar to those detailed above.

The Relation between the Work and Heat Production in Contracting Muscles.

The question as to the relation between the work accomplished by the contracting muscle and the amount of heat developed in contraction, must at present be confined to the consideration of the question as to whether under like conditions of irritation and arterial blood-supply the development of heat and change in form vary in direct proportion; for the elaborate and valuable conclusions drawn by Fick as to the laws governing muscular contraction are not applicable to a tetanus which accomplishes no work. To simplify this problem the influence exerted by the blood-current on the course and degree of contraction will be first considered.

If the circulation through the aorta is arrested only a few minutes before or simultaneously with the stimulation of the resting nerve and muscle, a maximal irritation produces a lift which is equal in height to that produced by the normal muscle. But while the energy of contraction is at first equal in both instances, the bloodless muscle rapidly lengthens during the stimulation, so that within one or two minutes after the commencement of irritation the muscle has regained its original length, even though there has been no decrease in the strength of the current applied to the nerve. The following figures illustrate the striking difference between the duration of shortening in the bloodless and normal muscle when subjected to equal stimuli.

a. 1st Tetanus.—*Circulation arrested.* Lift at commencement of irritation = 140 mm.
After tetanus of one minute's duration: lift = 11 mm; or, as 100 : 7.

2d. Tetanus.—*Circulation normal.* Lift at commencement of irritation = 140 mm.
After tetanus of seven minutes' duration: lift = 121 mm.; or, as 100 : 86.

b. 1st. Tetanus.—*Circulation arrested.* Lift at commencement of irritation = 145 mm.
After tetanus of five minutes' duration: lift = 55 mm.; or, as 100 : 38.

2d. Tetanus.—*Circulation normal.* Lift at commencement of irritation = 140 mm.
After tetanus of five minutes' duration: lift = 106 mm.; or, as 100 : 75.

c. 1st. Tetanus.—*Circulation normal.* Lift at commencement of irritation = 125 mm.
After tetanus of three minutes' duration; lift = 105 mm.; or, as 102 : 84.

2d. Tetanus.—*Circulation arrested.* Lift at commencement of of irritation = 110 mm.
After tetanus of one minute's duration: lift = 0 mm.; or, as 100 : 0.

The favorable influence exerted by the blood current on muscular contractility is also seen in cases where the aorta is first closed and then opened during an irritation. In such cases, if the muscles were not already fatigued by previous contraction, and if the aorta had not been closed more than a few minutes, when the circulation was restored during the stimulation, the muscle which had become almost entirely relaxed again shortened, though the degree of contraction never reached the point first attained at the commencement of the irritation.

If, on the other hand, the muscle had been already repeatedly tetanized, or if the blood supply had been arrested for a number of minutes, with the restoration of circulation there only occurs a very slow and incomplete return of contractility; showing that recovery only slowly occurs on restoring the blood supply to muscles injured by prolonged irritation and deprivation of blood. Finally, when the experiment as described above is performed on muscles highly fatigued by prolonged and repeated irritations, on reopening the circulation there is no return of contractile energy to the muscle, and the weight is not lifted above its height at the end of the period during which the circulation was closed.

If, in the first place, we compare the energies of contraction and heat-production in the stimulation of bloodless muscles, it is seen that in both the changes run in the same direction. At the commencement of irritation the muscle suddenly attains its highest degree of shortening, and simultaneously the heat-production, which is feeble during rest, is suddenly increased. It must be granted that the rapidity with which the thermometer indicates the maximum temperature falls far short of the rapidity with which the muscle shortens; but this does not necessarily indicate a slower development of heat, for even were the heat instantaneously developed, the low conductivity of the muscle mass and of the glass would require time before the increased temperature could be indicated by the thermometer.

Further, the relaxation, which is rapid in the first few minutes, becomes more and more gradual during the prolongation of the stimulation, and completes the analogy to the temperature changes, for the increase of temperature, which at first is rapid, becomes less and less as the stimulation is prolonged.

The following examples are selected from twenty experiments made on eight different dogs, and may be regarded as giving the possible extremes, as in the first example the evidences of fatigue were only slowly developed, while the muscle rapidly lost its contractility in the second.

Time after commencement of contraction in minutes . .		½	1	1½	2	2½	3	3½	4	4½	5	5½	6
I.													
Lift, in mms. .	145		75	65	65	65	65	55	55	52	52	52	52
Muscle temperature . .	39.59°												
Increase in each 30 seconds .		0.14	0.05	0.04	0.04	0.06	0.02	0.03	0.04	0.02	0.03	0.02	0.03
II.													
Lift, in mms. .	141	136	121	51	6	6							
Muscle temperature . .	39.25°												
Increase in each 30 seconds .		0.22	0.14	0.02	0.0	0.0							

The analogy of changes in the capabilities of the irritated muscle of developing heat and of contracting is still further preserved in the fact that, though both results of irritation have ceased to appear in a muscle fatigued by prolonged weak stimulation, when a maximal is then substituted for a sub-maximal irritation both heat development and shortening again reappear. The following examples serve to illustrate this point; both were obtained from the irritation of the same muscle, one during closure of the circulation, and one during normal blood supply. Their comparison serves to show how closely the muscular energy is dependent on the blood supply. The temperature is given for each thirty seconds after commencement of the tetanus:

1st Tetanus.—*Circulation Arrested:*

	Sub-maximal irritation.								Maximal irritation.	
Lift, in mms.	40		10		0			0	75	59
Muscle temperature	39.87°,	39.87,	39.92,	39.95,	39.97,	? 39.98,	? 39.98,	39.97	40.16,	40.21

2d Tetanus.—*Circulation Normal:*

	Sub-maximal irritation.												
Lift, in mms.	55		53		47		44				37		30
Muscle temperature	39.33°,	39.40,	39.47,	39.54,	39.60,	39.65,	39.69,	39.72,	39.74,	39.77,	39.79,	30.80,	39.82

	Maximal irritation.			
Lift, in mms.	89			80
Muscle temperature	40.08,	40.22,	40.30,	40.30

The following curves, fig. 4, *a.*, and fig. 4, *b.*, are constructed from tetanus 1 and 2, respectively; the shaded portion of the figure represents the course of the lift.

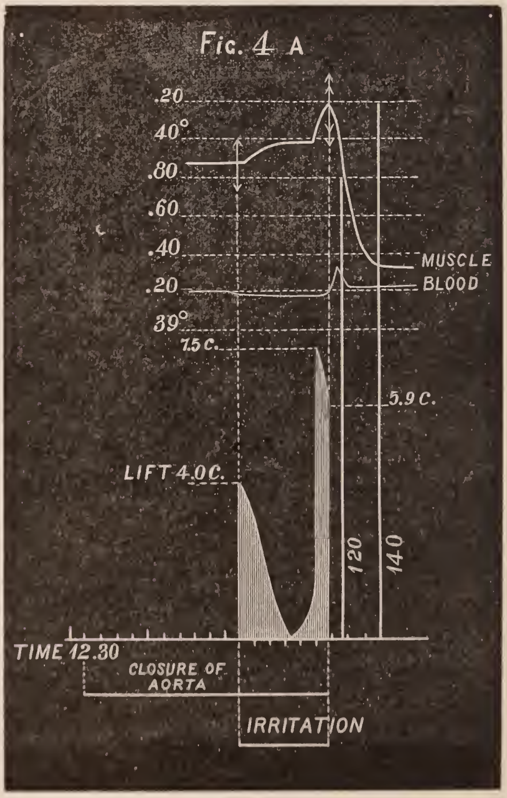

Fig. 4 A

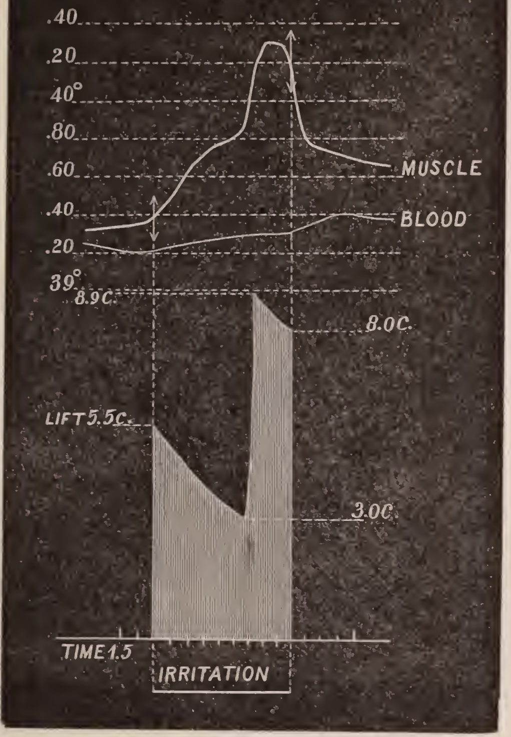

Fig. 4 B

In the muscle which still receives its normal supply of arterial blood the same relation still holds between the energies manifested in the development of heat and accomplishment of work: both forms of energy commence together and disappear together. But it cannot be concluded from this qualitative coincidence that the relation between the liberation of heat and contraction is the same in the bloodless and normal muscle. That with the presence of arterial blood this relation differs from that of the bloodless muscle is suggested by the observations of Heidenhain, which show that in prolonged work the muscle loses its power of developing heat before its contractility is lost; and consequently the invigorating action of the arterial blood supply must produce a different influence on the potential energies of the muscle from that exerted by the fatiguing contraction.

On the solution of this problem depends the most important deduction as to the internal processes in contracting muscle. For if the muscle in accomplishing equal degrees of work can develop unequal amounts of heat, the relation between these two phenomena can be but a limited one, and, as far as I can see, can only be explained in one of two ways: either through alteration in the relative inert mass of the muscle; or from the production by the irritation of two different kinds of internal rearrangement, one manifested by change in form, the other by development of heat.

In the muscle, as in all other machines, the position of the point of application of the force has an important influence on the result. And if, consistently with the conditions of life, changes may take place which permit of a more advantageous application of the force, it is readily conceivable that in spite of a diminished liberation of energy the actual amount of work may remain the same or even increase, as shown by the observations of Heidenhain already alluded to.

The second supposition, that in the irritated muscle two different chemical processes are simultaneously started by the stimulus, and that then each pursues an independent course, though at first sight contradictory, is really conceivable: for the muscle may preserve a condition of contraction, as in the so-called *contracture*, without appreciable development of heat; and, on the other hand, the resting muscle even when deprived of its arterial blood supply may increase in temperature.

The conditions under which the muscles contract in the living animal may be so varied by experiment as to permit of study of the influence of rest and fatigue, both with and without the arterial blood supply, and to lead to the hope that by numerous modifications of the conditions some idea may be formed as to the inter-dependence of heat production and contraction.

To accomplish this comparison of results attained under different conditions, I first subjected the muscle to two successive maximal irritations of one minute's duration; the first instance being in a fresh muscle deprived of its blood supply, the second with the aorta open.

Since we know that the muscle with normal blood supply surpasses the bloodless muscle both in its power of developing heat and of maintaining contraction, it therefore only still remains to be determined in what proportion, under the influence of the blood, the lift and temperature increase. And since in the normal muscle, through partial dissipation through the blood, the amount of heat developed remains unknown, this can only be accomplished through comparison of the increases of temperature in equal intervals of time; and as the degree of contraction in both cases does not lie within the same limits, only a mean between these can be used for comparison, a method which is not open to serious objection where the variations are within such narrow limits as in the example reported.

In the following examples the bloodless muscle was always irritated before the normal muscle: the temperature of the muscle was always above that of the blood.

I.

1st Tetanus.—*Circulation Arrested.*

Temperature increase at end of one minute = 0.36° C.
Lift at commencement of tetanus = 141 mm. } Mean Lift =
" end " = 121 mm. } 131 mm.

2d Tetanus.—*Circulation Normal.*

Temperature increase at end of one minute = 0.41° C.
Lift at commencement of tetanus = 140 mm. } Mean Lift =
" end " = 130 mm. } 135 mm.
Relation of temperature of bloodless and normal muscle = 1:1.14.

Relation of lift of bloodless and normal muscle = 1:1.03.

3d Tetanus.—*Circulation Arrested.*

Temperature increase at end of one minute = 0.28° C.

$\left.\begin{array}{l}\text{Lift at commencement of tetanus} = 138\text{ mm.}\\ \text{“ end “} = 105\text{ mm.}\end{array}\right\}$ Mean Lift = 121 mm.

4th Tetanus.—*Circulation Normal.*

Temperature increase at end of one minute = 0.35° C.

$\left.\begin{array}{l}\text{Lift at commencement of tetanus} = 134\text{ mm.}\\ \text{“ end “} = 111\text{ mm.}\end{array}\right\}$ Mean Lift = 122 mm.

Relation of temperature of bloodless and normal muscle = 1:1.25.

Relation of lift of bloodless and normal muscle = 1:1.00.

II.

1st Tetanus.—*Circulation Arrested.*

Temperature increase at end of one minute = 0.30° C.

$\left.\begin{array}{l}\text{Lift at commencement of tetanus} = 70\text{ mm.}\\ \text{“ end “} = 30\text{ mm.}\end{array}\right\}$ Mean Lift = 50 mm.

2d Tetanus.—*Circulation Normal.*

Temperature increase at end of one minute = 0.46° C.

$\left.\begin{array}{l}\text{Lift at commencement of tetanus} = 59\text{ mm.}\\ \text{“ end “} = 61\text{ mm.}\end{array}\right\}$ Mean Lift = 60 mm.

Relation of temperature of bloodless and normal muscle = 1:1.53.

Relation of lift of bloodless and normal muscle = 1:1.20.

III.

1st Tetanus.—*Circulation Arrested.*

Temperature increase at end of one minute = 0.29° C.

$\left.\begin{array}{l}\text{Lift at commencement of tetanus} = 86\text{ mm.}\\ \text{“ end “} = 55\text{ mm.}\end{array}\right\}$ Mean Lift = 70.5 mm.

2d Tetanus.—*Circulation Normal.*

Temperature increase at end of one minute = 0.37° C.

$\left.\begin{array}{l}\text{Lift at commencement of tetanus} = 85\text{ mm.}\\ \text{“ end “} = 82\text{ mm.}\end{array}\right\}$ Mean Lift = 83.5 mm.

Relation of temperature of bloodless and normal muscle = 1:1.24.

Relation of lift of bloodless and normal muscle = 1:1.18.

If instead of the above quotients of temperature-increase it were possible to estimate the amount of heat developed, there can be no doubt but that it would be found that the excess of heat production in the normal over the bloodless muscle would be even greater than appears in the above experiments, since my earlier experiments showed that even in the first minutes of muscular contraction the venous blood coming from the muscle is warmer than the arterial supply going to it. Nevertheless, in spite of the unfavorable condition of the calculation it is clear that the resting muscle draws from its arterial blood supply a greater heat-producing than contracting energy.

The experiments above detailed, however, do not yet warrant the conclusion that more heat is developed in normal contracting muscle from the combustion of the matter stored up in its interior during its period of rest than would be the case in equal degrees of contraction in the bloodless muscle; they, however, indicate the manner in which, to be conclusive, new experiments must be made.

The relation between the temperature and lift of the muscle is, as a rule, inverse, when, instead of a fresh muscle, one already fatigued by prolonged irritation and deprivation of blood supply is again thrown into contraction after renewal of circulation.

Thus, when the capability of producing heat has been greatly reduced by prolonged irritation and repeated closure and opening of the aorta, the reopening of the circulation often produces a marked increase in the energy of the contraction, as shown by a higher lift, with but slight change in the temperature.

The following experiment and curve (fig. 5) will serve to make this clear. During the nine minutes through which the irritation lasted the circulation was at first arrested, then opened, again closed, and finally again opened.

a. Circulation Arrested.

Time after commencement of contraction		½	1	min.
Muscle temperature	39.14° C	39.37	39.40	
Lift	141	86	11	mm.

b. Circulation Normal.

Time after commencement of irritation	1½	2.	2½	3	3½	4	4½	min.
Blood Temperature	39.12	?	39.29	39.32	39.31	39.30	39.28	
Muscle Temperature	39.43	39.48	39.48	39.44	39.40	39.37	39.37	
Lift	106	109	109	110	110	110		mm.

c. Circulation Arrested.

Time	5	5½	6	6½	7	min.
Muscle Temperature	39.37	39.36	39.39	39.41	39.43	
Lift	81	9	8	9		mm.

d. Circulation Normal.

Time	7½	8	8½	9	min.
Temperature of blood	39.34	39.39	39.37	39.35	
" " muscle	39.44	39.43	39.40	39.40°	
Lift	9	96		106	mm.
One Minute after End of Tetanus.					
Temperature of blood	39.40				
" " muscle	39.34				
Three Minutes after End of Tetanus.					
Temperature of blood	39.37				
" " muscle	39.35				

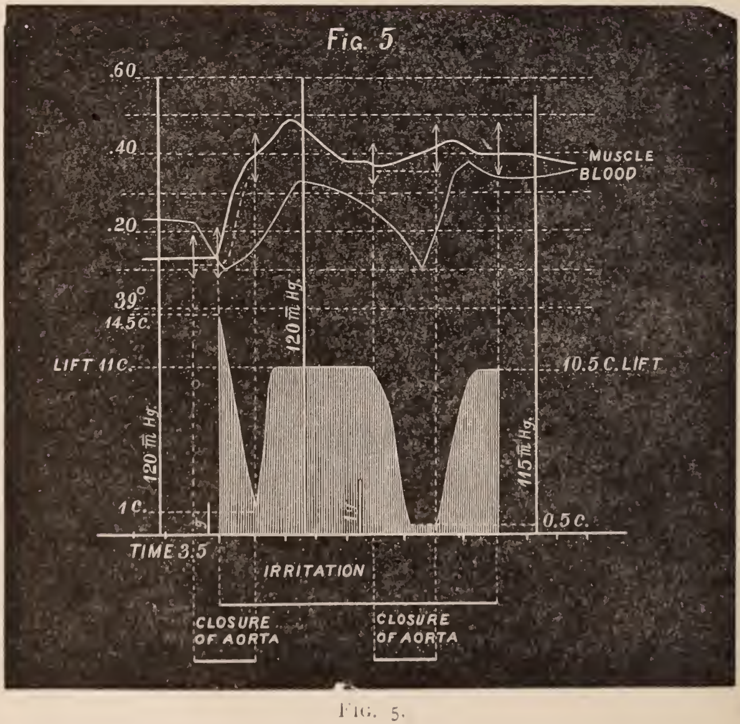

FIG. 5.

In the second period (*b*) of the above experiment the excess of muscle temperature over that of the blood sank in three and a half minutes from + 0.31° C. to + 0.09°, while the lift increased from 106 mm. to 110 mm.; in the fourth period (*d*) the excess of muscle temperature decreased in two minutes from 0.10° C. to 0.05°, while the lift during the same period increased from 9 mm. to 106 mm. In the second period the average decrease in excess of muscle temperature was 0.06° C. for each minute, and in the fourth period 0.025° C., while after the end of irritation it amounted to 0.03° C.

The results of the above experiments correspond with the conclusions drawn by Heidenhain from investigations made on the excised frog's muscle, but the results which were obtained have not been invariably uniform. Among the deviations, the following experiment furnishes an example: Before the tetanus occurred whose course is here described, the muscle had been already subjected to repeated irritations, although immediately preceding the final contraction it had an interval of rest of ten minutes, during which it was supplied with arterial blood for five minutes, and again deprived of blood for five minutes. It was then tetanized for eleven minutes, among the first three of which the aorta remained closed, and was then opened and remained open to the end of the experiment. The following curve represents the course of the experiment.

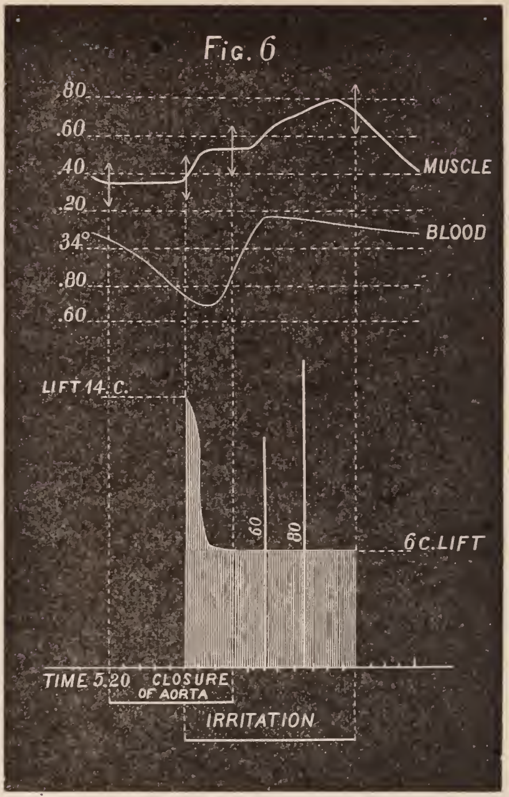

FIG 6.

In the first portion of the experiment, the result coincides with what had previously been found. When, however, the circulation was restored, no increase occurred in the lift, but nevertheless the temperature, after a slight delay, gradually rose 0.23° C. during the following six minutes, or as great an increase as occurred at the commencement of the irritation.

In conclusion I must regret that circumstances prevented my carrying the above study to its completion, but trust that the results above recorded will assist others in extending a subject which must yield most valuable results as to the relations between the blood and the internal vital processes in muscle, and as to the share of the heat production in muscular contraction in maintaining the bodily temperature.

MULTIPLE NEURITIS.*

By S. G. WEBBER, M.D.,

BOSTON.

NEURITIS has lately assumed an importance which formerly was not accorded it. Local traumatic and rheumatic neuritis have been observed many times, and their symptoms have been well described. The grosser and more marked anatomical changes were well known to the profession years ago. The secondary degenerations which arise in the peripheral ends of divided nerves have been known since Waller described them. More recently various trophic changes in the skin have been correctly referred to disease of the nerves.

It is only within a few years that a more general inflammation of the nerves has been recognized. As has frequently happened, the disease had been seen and cases reported at intervals,—in 1864 by Duménil, Lancereaux in 1870 or 1871, and Eichhorst in 1877,—but attention was not specially attracted to the subject until Joffroy in 1879, Leyden in 1880, and Stewart in 1881, had each described cases. Even then it was some months before the possibility of a general diffused neuritis was fully recognized, if indeed it is as yet. In 1882 Caspari published his article "Zur Casuistik der Neuritiden." In 1883 appeared Pierson's "Uber Polyneuritis acuta" (multiple neuritis), an article by Strümpell, one by Vierordt, and another by Müller in the *Archiv für Psychiatrie*, etc.

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* Read before the American Neurological Association, June, 1884.

Up to this time I have found the records of six cases without autopsies and twelve cases with autopsies—one, however, very brief. As so few cases have been reported, the following reports and summary may be of sufficient interest to claim the attention of this Society.

Case 1.—Thos. McG., æt. twenty-two. Entered Dec. 7, 1883. Was well until one year ago. Since then he has had pains in the abdomen and back, at times lasting several hours, and of a severe darting character. No digestive disturbance. Constipation. Said he had eaten some poorly cooked ham, after which he and others had headache. Nov. 27th he took cold, and Dec. 2d had pains in the legs, and the next day in the arms, a constant aching of the muscles with soreness, worse in the right arm and left leg. Swelling of arms and legs. No pain in back on admission. Chilly and feverish much of the time. Severe headache on Dec. 2d and 3d; dizzy on rising. Vision good; some tinnitus.

On examination it was found that there was tenderness over the biceps on both sides, over the muscles of the right forearm, and over the left radial nerve; not over the other nerves of the arms. Tenderness also over calves and thighs on both sides, over peroneal nerve and sciatics. Both hands and both feet were swollen; no swelling of arms above the wrist, though the patient thought there was. Face was much swollen. There was no special tenderness of the joints, and no swelling of joints, excepting those of the hands and feet, which were included in the general swelling of those parts.

Both arms were contracted in a partially flexed position, the left arm being most contracted. All the muscles below the hips were more or less paralyzed.

Plantar reflex was most marked on the left. Patellar reflex was absent on both sides; no ankle clonus.

Urine was normal in color, acid. Sp. gr. 1025. No albumen; sediment—a large amount of amorphous urates.

Respiratory sounds were normal throughout both lungs.

The faradic reaction was not tested until after the patient had been in hospital several days; then it was found that the tibialis anticus, peronei, and vastus externus on both sides responded feebly even to a strong curve. The rectus femoris and vastus internus did not respond at all. The interossei reacted moderately well; the extensors in forearm, supinator longus and biceps, reacted scarcely at all; the triceps, deltoid, and flexors responded somewhat better. The facial muscles responded with very nearly the same strength as in health.

The treatment was salicylic acid, iodide of potassium, electricity. The swelling, pain, contraction, and paralysis gradually yielded, and after about two months and a half he could get about fairly well. The œdema reappeared occasionally. The patellar tendon reflex did not return before he was discharged. At times there was profuse perspiration. During the first three weeks the temperature was above normal, once reaching 104.5°. After that the temperature was generally normal. The pulse was almost constantly ninety or above.

April 7th.—He was seen. There was no contracture remaining. No tenderness of legs or arms. There was absence of patellar reflex on the left, it was very slight on the right. He considered himself well and strong.

Case 2.—I. R. æt twenty-eight, entered the hospital, October 26th. She was well and strong until two months ago. She denied syphilis; had done considerable hard work. Two months ago while at the beach she "took cold" during her catamenia, after which she began to have pain in the abdomen; this was followed by numbness and loss of sensation. There was a girdle sensation about the waist which later disappeared. No pain in the back. The numbness gradually passed downwards, leaving the abdomen, and on entrance was confined to the knees and legs. Occasionally there were sharp pains in the legs, at other times a dead aching. She could move her legs and feet, but the legs were slightly flexed at the knees, and attempts to extend them or to sit up caused feeling of distress about the heart. She was unable to stand. The pain in the legs was chiefly in the knees, calves, and feet, and was worse at night. There was a slight numbness in the hands. There was no headache, nor dizziness; vision was good, there had been no diplopia; once a slight tinnitus. Occasionally a slight cough, pain in chest and dyspnœa; no palpitation. Catamenia were regular, not painful, for last two months delayed three or four days. Appetite and digestion were good, bowels regular, micturition normal.

Nothing abnormal was found in lungs or heart. There was general tenderness over the muscles of the legs and thighs, and in the arms near the wrist, and over the popliteal and sciatic nerves. There was diminution of tactile sensation in the left leg as compared with the right. The plantar reflex was fairly good; patellar tendon reflex was absent on both sides.

To the faradic current on the right, the tibialis anticus acted fairly well, the peroneus, rectus femoris, vastus internus, and gastrocnemius not quite so well. On the left, the tibialis anticus acted best, the rectus femoris gave scarcely any reaction, the gastrocnemius also responded very sluggishly.

With the galvanic current on the right the tibialis anticus responded readily and gave a quick contraction to fourteen cells; the peroneus gave a slow but strong contraction to twelve cells ; the gastrocnemius responded very sluggishly to sixteen cells. Very nearly the same reactions were obtained on the left.

The pain and tenderness increased in the legs and extended to the upper extremities, the nails and ends of the fingers being first affected. There was tenderness over all the larger nerve-trunks in both arms.

At one time the cough had a barking sound and seemed to be somewhat hysterical in character.

The treatment was salicylic acid, ten to twelve grains every three hours ; blisters over tender nerves in the arms ; morphia to relieve pain ; five-per-cent. solution carbolic acid on compresses over painful muscles. This last application gave considerable relief to the pain.

Nov. 16th.—Most of the pain had left the legs and fingers ; the fingers were anæsthetic, their ends tender on pressure ; pressure to calves also caused pain. The tenderness had disappeared from over the ulnar and radial nerve, was still considerable over the median and popliteal on both sides, slight over the left sciatic.

Nov. 21st.—The pain was entirely gone from the legs and arms, but the legs were still partially flexed and could not be extended without pain.

Jan. 19, 1884.—Tenderness was quite gone from the fingers, except the forefingers. The grasp of the hands was of moderate strength and the same on both sides. The calves of both legs were still tender. The peroneus did not respond to the faradic current on either side. The gastrocnemii and recti femoris required strong currents. The vastus internus did not respond on the left ; it required a strong current on the right.

There was contraction of the flexor muscles of the legs, so that the legs were bent at nearly right angles to the thighs ; the feet were also in extension and an effort to rectify the position of either the legs or feet caused considerable pain. By means of massage, extension by weights, by elastic bands, and with the aid of electricity the vicious positions were rectified.

Five months after admission she was able to walk with assistance, the heels did not touch the floor in standing, and she could not stand upright, stooping forward. The calves were still tender to pressure, and there was tenderness over the popliteal nerve; patellar tendon reflex was absent.

A month later she was able to walk without assistance and helped in the work of the ward.

CASE 3.—Caroline M., æt. thirty-four. Was received Jan. 18, 1884, with a family history of consumption. She was said to have had rheumatism for several years; and for a long time had had palpitation and dyspnœa. Had been somewhat addicted to the use of alcohol. No cough, micturition normal. Cata. regular. She had had an attack of vomiting three months previous to present illness.

In the earlier part of illness, which began about four weeks before entrance, there were pain and numbness in the legs; this improved somewhat, then the pain returned. There was no disturbance of motion excepting as explained by the pain. This account was given from recollection by her physician after her death.

On admission she complained much of pain, chiefly in the joints and back, which had troubled her for three weeks; was unable to turn herself in bed on account of pain; she could not bear to have the ends of the fingers touched. She perspired profusely; was delirious.

Tenderness over forearms, most marked on right; over median nerve on left, median and ulnar nerves on right, and over right clavicle. The calves were also tender, as well as the popliteal and sciatic nerves in legs. Entire paralysis of hands and fingers, legs and feet, and perhaps of thighs. Anæsthesia to pricking and touch in left leg and thighs. Œdema of ankles, feet, legs, and hands. Absence of patellar tendon reflex and plantar reflex. She says that occasionally she chokes on swallowing, but at visit swallowed without difficulty. Voice seemed to be changed but was not nasal. The pain seemed to become less severe; at seven P.M., Jan. 19th, she died. No autopsy.

CASE 4.—Mary N., entered the City Hospital Nov. 13th; no special family tendencies; no severe illness. Three years ago had a bubo in groin, and husband had gonorrhœa at the same time. Had two children; no miscarriages; first child died at four months of age; had indulged in excessive use of alcohol, and four weeks before entrance had drunk much, and had a severe attack of vomiting lasting two days, was delirious three or four days, and saw all sorts of things. For some days before entrance, how long is not mentioned in the records, she had slept poorly on account of dull aching pains in lower extremities; had numbness in hands and forearms. The pain in limbs became severe, she could not turn over in bed without assistance, her knees were drawn up, and attempts to straighten the legs caused severe pain. There was loss of power in extensors of the wrist and hands, and the fingers could not be shut perfectly. There was general tenderness over the muscles of the extremities. Pressure on right over the radial, median, and ulnar nerves, on the left over the median, caused much pain. In the legs pressure was painful over the popliteal, sciatic, and crural nerves on both sides. The voice was hoarse, and she complained of sore throat; the fauces were of a rather deeper purple tint than normal. Her general appearance indicated so much suffering that a complete examination was not made. Inquiries as to a sore throat previous to admission, and as to the possibility of diphtheria, showed that she had not suffered from any such disease.

Two or three days later it is noted that there was considerable anæsthesia of fingers and feet; no patellar tendon reflex. The pain was so severe that morphia was given to control it. The paralysis became more complete, the anæsthesia increased, the pain was rather less, or was controlled by the morphia. The voice was reduced to a whisper, and even whispering necessitated great exertion. Urine and fæces passed involuntarily.

Nov. 28th.—There was considerable difficulty in breathing, respiration rapid; less pain, much thirst; was said to swallow perfectly well. Air entered the apex of right lung less readily than the left; an occasional râle was heard; percussion resonance not so good on right. Respiration was accomplished by means of the upper ribs and the accessory muscles; there was paralysis of the diaphragm. The respiratory murmur could not be heard lower than the lower edge of the sixth rib. She died seventeen days after entrance.

Autopsy seventy-two hours after death by Dr. Gannett. The lungs showed on section small nodules, grayish and granular, denser than the lung substance. These nodules, about the size of a pin's head, could be felt by pressure from without. On pressure of cut surface, numerous points of pus were to be seen. The bronchial mucous membrane, as well as that of the larynx, trachea, and pharynx, was covered with a puriform fluid. The kidneys were congested, and there was acute purulent pyelo-nephritis. On section of the liver the central parts of the acini were distinct as brownish areas one mm. in diameter; the peripheral parts were opaque reddish gray. The microscope showed central fatty infiltration, pigmentation, and cloudiness of cells of remaining portion.

The brain and spinal cord seemed to be healthy, both externally and on section.

The brachial plexus on both sides with their larger branches as far as the wrist, the sciatics with branches as far as the heel, the crurals as far as the knee, the phrenics, and vagi, were removed.

These various nerves showed nothing abnormal in their gross appearance; there was no enlargement nor undue redness.

Microscopic Examination.—Numerous sections of the cord were made in various parts, chiefly at the cervical and lumbar enlargement; more than a hundred were carefully examined. The cells of the anterior cornua showed no variation from the normal in number nor in character; the white and gray substance seemed to be healthy; there was no undue injection nor disease of the blood-vessels.

Portions of the nerves were put into a $\frac{2}{3}$-% solution of osmic acid for twenty-four hours, were then placed in a solution of picro-carmine for two days, then placed in glycerine until they could be examined. The parts thus treated were the upper part of both brachial plexuses; the distal ends of the median, ulnar, and radial on both sides; the lower end of the longest branch of both sciatic systems, from near the heel; the right pneumogastric and right recurrent laryngeal; the small portion of the pneumogastric from near the stomach; the upper part of the right phrenic. (The rest of the phrenic had been removed with the thoracic viscera, and could not be obtained.) Except the phrenic, and that part of the pneumogastric from near the stomach, and the upper parts of the brachial plexus, every nerve thus examined was much diseased. The lower ends of the sciatic system were most extensively diseased. The recurrent laryngeal and the vagus, just below where it is given off, were least affected. In one brachial plexus, among many specimens examined, one degenerated nerve fibre was found; in these specimens from the brachial plexuses were found some fibres which seemed to be in the very first stage of inflammation, but most of the fibres were healthy. The distal ends of the median, ulnar, and radial on both sides were much diseased, but less so than the nerves from the leg.

The portions of nerve not placed in the osmic acid were put into Müller's fluid.

The simplest change found was in the medullary sheath at Ranvier's constrictions, without break in the axis, and, so far as I could judge, without increase of nuclei.

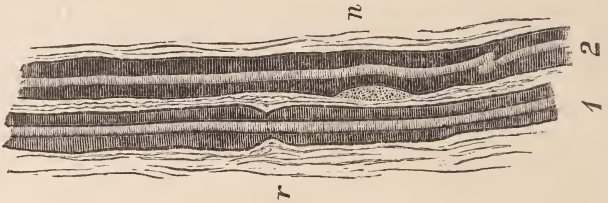

Fig. 1.—Healthy nerve fibres, osmic acid preparation. *r*, constriction of Ranvier; *n*, nucleus. The axis cylinder is bent on itself in one place.

The changes in the distal ends of the nerves were very similar or identical with what is found in secondary degeneration after section of a nerve. The medullary sheath was divided into fragments of greater or less extent; the axis-cylinder was discontinuous or destroyed, remaining when the medullary sheath was only slightly changed. The fragments of the medullary sheath were finally reduced to very small granular débris, and much of this was absorbed. In the fibres which were most altered was found an increase of nuclei; and apparently small masses of protoplasm, not nuclei, were tinted in the midst of the granular débris.

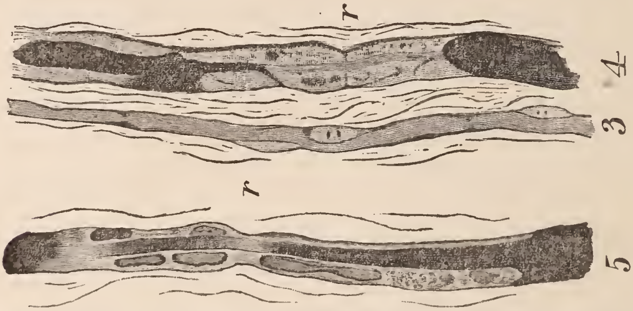

Fig. 2.—At 3 a thin fibre, with three nuclei in or adherent to it; at 4 and 5 inflammatory changes just beginning in the medullary sheath at the annular constrictions *r*.

On cross-section of portions hardened in Müller's fluid and tinted with carmine, the distal ends, especially of the branches of the sciatic, showed almost no nerve fibres with axis-cylinders; the sheath was either filled with granular material or had collapsed. There were many small masses, either of fibres or nuclei, which were tinted almost uniformly a deep red. Nearer the root of the

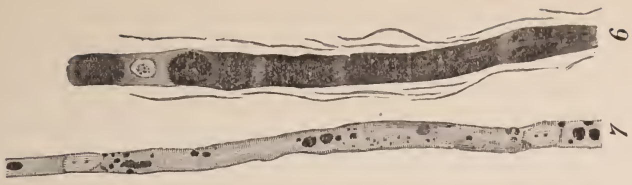

FIG. 3. The first segmentation of secondary degeneration at 6; extreme degree of this degeneration, only a few points or dots of myeline showing, 7.

nerves, axis-cylinders were more numerous, and specimens yet nearer the origin showed a large preponderance of healthy nerve fibres. In the sciatic the sections from the highest part of the nerve which had been preserved were still diseased in a small proportion of their fibres.

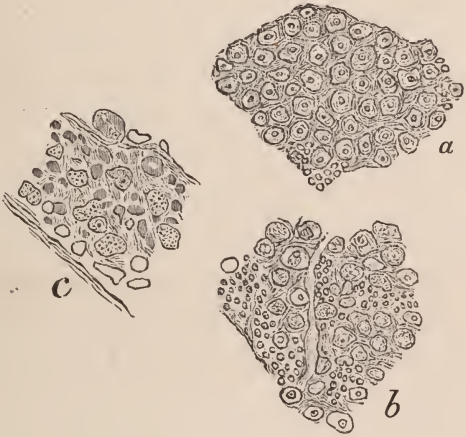

FIG. 4.—Transverse sections of nerve hardened in Müller's fluid, stained with carmine. *a*, nearly healthy from brachial plexus; *b*, much diseased from radial; *c*, extreme degeneration from sciatic.

These few cases are sufficient as examples of this disease. It happened that during the fall and winter of '83–'84, an unusually large number of cases of multiple neuritis entered the City Hospital—eighteen. I saw one in private practice, and a second in which the diagnosis was somewhat doubtful, between neuritis and infantile paralysis. Fourteen of those at the City Hospital came under my observation, nine of these on their entrance, five as remaining when I took the service from Dr. Denny.

Of all the patients the youngest was nine years old (a private patient). This is the only child in the list. The oldest was fifty-one years. More than half were between twenty and thirty years of age.

Where the commencement of the disease could be fixed with any degree of certainty, there was an interval of from one week to four months before the patient entered the hospital. The stay in hospital varied from two days (patient died) to six months; three or four months in most cases, and even after four months' stay only a few patients left the hospital able to engage in their usual occupations; generally there was still great weakness of the limbs.

In most cases no cause could be given for the disease; in five cases exposure to wet and cold, "catching cold," was thought by the patient to be the cause of his sickness.

Nine were females. Ten were males.

There were three deaths: two of the cases in which death occurred have been reported.

Disturbance of sensation is one of the most constant and prominent symptoms. A tingling or "sleepy" feeling may precede the more severe pain; within a comparatively short time the pain becomes severe; it is confined to one nerve district or may extend to several; sometimes the patient can mark the course of the nerve by the pain. The character of the pain, aching, boring, shooting, burning, varies in different cases. With the pain there is great hyperæsthesia of the skin and tenderness of the muscles, pressure causing excessive pain. Pressure over the nerve trunks gives rise to more acute pain than when it is applied to the muscles.

When the hyperæsthesia has subsided sufficiently there will be found diminution of tactile and other sensations. The special senses have only rarely been found affected.

Exceptionally the pain and tenderness have been very slight or wanting.

Motion is early disturbed, at first a stiffness, in part due to pain ; later paralysis, more or less complete. The muscles are not affected equally, but the paralysis is distributed according to the nerves attacked.

Within a comparatively short time the limbs are contracted, semi-flexed, and sometimes the contraction may become extreme, the heels may be drawn up to the thighs, and the knees nearly to the chest.

The electrical reactions of the muscles and nerves show the changes due to their respective degeneration.

A careful examination of the reflexes, deep and superficial, was made by Dr. Knapp, house officer at the hospital. He found that the various tendon reflexes were generally absent, the patellar tendon reflex being slightly marked in only two out of thirteen patients ; the radial and ulnar periosteal reflexes were present in only one or two patients.

The cutaneous reflexes were less disturbed. The plantar reflex was present in eight patients, in two of these rather exaggerated ; was absent in five. The cremaster reflex was present in five out of eight males ; the abdominal reflex was present in four, absent in nine.

In four other patients not examined by Dr. Knapp, I found the patellar tendon reflex absent ; the plantar reflex was also absent in three.

The temperature and pulse were rather elevated in the earlier stages of the disease, and in the two fatal cases throughout the period when they were under observation. Later the temperature dropped nearly or quite to the normal, but the pulse rate continued rather high.

In a few cases a mild delirium was noticed, most marked at night ; in one or two women there seemed an hysterical disposition, but neither the delirium nor hysteria were of more than secondary importance.

There was atrophy of the muscles, and in some patients great general wasting of tissue and loss of flesh.

In other patients there was œdema of the limbs and even of the face; this œdema in case 1 affected the hands and feet, in case 3 was more general, and might have led to doubt as to diagnosis, simulating rheumatism.

Excessive sweating was observed in some of the patients, in this respect also simulating rheumatism.

After several weeks' continuance of the disease the skin of the fingers in one patient assumed the glossy appearance peculiar to the trophic disturbance seen in some nerve lesions.

One patient had abscesses in the parotid region during convalescence.

The disease began most frequently in the legs, was sometimes confined to them, but often extended to the arms. In fatal cases its progress was rapid, the respiratory muscles became paralyzed, the respiration was diaphragmatic just before death; the voice was affected. and deglutition was abnormal; the patient died of apnœa, or in case 4, perhaps also from the entrance of food into the lungs. In case 4 death occurred seventeen days after entrance. Case 3 had been sick about four weeks before entrance, though the report is not entirely clear, as she seems to have improved at one time and then became worse again.

Now that the peculiar symptoms of multiple neuritis have been clearly pointed out, the diagnosis is not difficult in well-marked cases, but in light cases might be difficult.

It is to be diagnosticated from anterior poliomyelitis by the pain and hyperæsthesia, the tenderness over nerve trunks, the diminution of sensation, the high pulse rate; by its more gradual onset from the acute form of poliomyelitis; it is less common for the latter to invade all four extremities.

Progressive muscular atrophy does not have the sensory disturbances; the electrical changes are not the same. In lead-paralysis also the sensory disturbances are usually less marked, and the progress is slower.

Multiple neuritis may be mistaken for rheumatic fever, and when there are œdema of the limbs and profuse sweating, as in the first and third cases, there may be reason for making such a mistake. The course of the disease would soon show the mistake. Spinal meningitis will be recognized by the greater amount of pain in the back, by passive motion increasing this pain, by the spasm if present, and by the absence of other peculiarities of neuritis.

Déjérine (Étude sur le névro-tabes périphérique—ataxie locomotrice par névrites périphériques, avec intégrité absolue des racines postérieurs, des ganglions spinaux et de la mœlle épinière—*Arch. de Physiol.*, 1884, p. 231) has recently reported a case which was supposed to be locomotor ataxia, where after death no other lesion than neuritis of the peripheral nerves was found.

The disease known in Japan as *kak-ke* has been described by Scheube, who has carefully examined the nervous system in twenty patients. He found neuritis in every case; slight lesion of the cord, not sufficient to explain the symptoms, in only one case. He believes a specific poison is the cause of the disease.

Recently Dr. J. B. de Lacerda has published a monograph (see *Science*, March 14, 1884, p. 331. Etiologia e genesis de beri-beri. Pelo Dr. J. B. de Lacerda. Rio de Janeiro, 1884), in which he claims to have discovered the bacillus of beri-beri, and by injecting liquid containing this bacillus into rabbits and Guinea-pigs he produced a similar disease. He also found similar bacilli in rice, and cultivating he produced the organisms, and injecting these into animals produced the disease.

The nature of the pathological process in these cases was generally believed to be an inflammation of the nerves. But at a meeting of the Section for Psychiatry and Neurology of the Versammlung deutscher Naturforscher und Aerzte in Freiburg, held last Sept., Erb made two suggestions (*Allgemeine Zeitsch. f. Psych.*, etc., xl., 1884, p. 834), which he thought deserved more consideration:

"1. It is possible and probable that purely functional disturbances of the trophic centres in the spinal cord produce anatomical disturbances (degenerative atrophy) in the periphery (the motor nerves and muscles).

"2. It is possible that besides the complete destruction and separation of the trophic centres, yet other pathological processes occur in them which need not always go so far as to show themselves in the form of total degenerative atrophy of the peripheral regions of these centres."

These suppositions seem to have been answered sufficiently at that meeting by Strümpell, Schultze, and Seeligmüller, who show that Erb's hypotheses are less likely to be correct than that which refers the lesion to a peripheral neuritis. Erb replies that his hypothesis is only met by an hypothesis.

In the case just reported the degeneration in the peripheral ends is simply the degeneration due to the separation of a nerve from its trophic centre, the Wallerian degeneration (fig. 4 shows the first segmentation, fig. 5 shows the extreme degree of change). As specimens from nearer the origin of the nerves, this degeneration was less marked, and a larger number of healthy nerve fibres appeared. In those taken from the brachial plexus the nerves show change only about the constrictions of Ranvier. The first change is not segmentation, but a disturbance of the medullary sheath at these points (figs. 2 and 3).

The same change was seen in a specimen from a Guineapig, where the nerve was in the first stage of inflammation.

Gombault (*Archives de Néurologie*, t. I., 1880, pp. 11–177) has studied the changes in neuritis produced artificially in Guinea-pigs by mixing lead with their food. He finds that the seat of commencing change in the segments of a nerve fibre is at the end of the segment, the axis-cylinder remaining intact. He concludes that the difference between inflammation and Wallerian degeneration is: (1) that in the same nerve fibre the lesion is discontinuous, it attacks certain inter-annular segments to the exclusion of the others; (2) the axis-cylinder is not interrupted, the myeline sheath and protoplasm are alone affected. It is a true parenchymatous inflammation; it may terminate in restoration of the fibre, or the axis-cylinder may be at length destroyed, and then the Wallerian degeneration follows in the distal end of the nerve fibre.

Gombault thinks this peri-axillary neuritis does not have its origin in a change of trophic action of the spinal centres.

Considering the changes found about the annular constrictions, it is reasonable to consider the disease a neuritis, not dependent upon changes in the spinal cord. The fact that the distal extremities of the nerves were most affected, and the change diminished toward the spinal centres and ceased in some nerves long before the spinal cord was reached, would also lead to the conclusion that the disease was not dependent upon changes in the cord.

In most cases the patients recovered more or less completely, but recovery was very slow in severe cases. It required much time and patience to straighten the contracted limbs, and strength was regained very gradually.

I am not sure that any treatment shortened the attack. Salicylic acid seemed to cut short the pain in some cases, and when it returned it was not quite so severe. It was necessary to give morphia to relieve pain. A 4 % to 5 %

solution of carbolic acid applied on cloths to the painful limbs was of decided advantage. One patient thought ice to the spine grateful, another preferred hot water, complaining that ice increased the pain in the limbs. Blisters over nerves seemed to be of advantage in one or two cases where used, especially in a case where the hyperæsthesia was very persistent.

It is well to try to avoid or diminish the extent of the contractions by position or gentle extension of the limbs, a sand-bag to rest the feet against, etc., for the contractions are difficult to overcome. To do so requires a persevering use of massage, extension, and electricity. These agents are to be used a long time to promote a return of power.

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CARCINOMA OF THE PERITONEUM.

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MOST malignant new-formations of the peritoneum are carcinomatous. Those of other nature are usually secondary, either by continuity or metastasis. Enchondroma has been thus observed, exhibiting a feature of malignancy unusual, though not without precedent (Wood, *Lancet*, 1881, i., 249). Myxo-sarcoma and sarcoma have also been encountered, both as primary and as secondary growths. Primary peritoneal sarcoma has been reported by Kelsch and Wannenbroncq,[1] Berlioz,[2] Weiss,[3] and others.

Carcinoma of the peritoneum may be primary or secondary. Of late years there has been not a little confusion concerning primary carcinoma of the peritoneum. From time to time peritoneal carcinoma has been observed where every evidence of its primary development was present at the necropsy. At the same time the impossibility of such occurrence continued to be urged by the upholders of the germinal lamina theory of tumor formation, by whom it has been insisted that carcinoma, a new-growth dependent for at least a portion of its structure upon tissues derived directly from the epiblastic layer of the blastoderm, could not be developed from the peritoneum, a purely mesoblastic structure.

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[1] *Prog. méd.*, ix., 1881, 729.

[2] *Jour. de la soc. de méd.*, etc., *de l'Isère*, 1879, 89.

[3] *Aertz. Berl. d. k. k. Allg. Krankenh, zu Prag.*, 1882, 128.

Various explanations of the apparent existence of primary cancer of this membrane have been offered. Some have assumed it to be really secondary to primary undetected formations in epithelial structures. Klebs suggested an infection of the peritoneum with epithelial elements, rather than by a carcinomatous new-growth originating in the peritoneal endothelium, leaving to future investigation the task of determining the route by which the epithelial elements are brought to the peritoneum.[1] These pathological difficulties have, fortunately, been greatly relieved by recent embryological discoveries, whereby practical experience may be reconciled with the prevailing theory. The investigations of Hertwig and Balfour show that the lining membrane of the peritoneum is derived from the hypoblast, and that it is, therefore, a true epithelial structure.[2] There is, therefore, no difficulty in concluding that carcinoma not only may but does occur as a primary peritoneal new-formation.

Varieties of Peritoneal Carcinoma.—The cellular elements predominate in proportion to the rapidity of growth of peritoneal cancer. In the most rapidly fatal varieties the growth may have the softness of brain substance. Acute miliary carcinoma is probably always medullary. In less acute primary carcinoma both this and the fibrous (scirrhous) and colloid forms occur. The connective-tissue element will be most abundant in the slower forms and hard leathery bands of malignant infiltration will be developed. Colloid cancer, usually indicative of a more chronic course, may complicate both the medullary and scirrhous forms. Petrina [3] records forty cases of peritoneal carcinoma. Of these fourteen were primary, nine medullary, and five scirrhous (fibrous); twenty-six were secondary—fourteen medullary, ten scirrhous, two colloid. Chuquet [4] found that of the cases collected by him, two thirds were primary. (He also found colloid carcinoma to be the most frequent. It is more correct to speak of this as a degenerative condition of cancer than as a distinct variety, and to apply the term colloid cancer only when the colloid change is predominating. To a limited extent it can be discovered in a large proportion of carcinomas.) Carcinoma may affect the peritoneum secondarily by continuity from other organs or tissues, and by metastasis. Melanotic cancer may thus rarely develop.

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[1] *Handb. d. path. Anat.*, 2d part, 1869, p. 337.

[2] Ziegler's "Path. Anat.," part 2. Translated by McAllister. Lond., p. 123.

[3] *Viertelj. f. d. prak. Heilk.* Prag., 1872, 2 B., 541.

[4] "Thèse de Paris," No, 548, 1879.

Symptoms and Course.—Primary peritoneal cancer may develop very insidiously, and has an indefinite and irregular course. Pain commonly first attracts the attention of the patient. This may become nearly constant or paroxysmal, most intense in some fixed region, and radiating thence to other parts of the abdominal cavity, or to the chest, shoulders, or back, or down the thighs. It is variously described as stabbing, stinging, burning, or as a dull, heavy sensation. As a rule it gradually increases in severity until it becomes a source of unending distress or even agony. It may, however, not be present during the earlier stages, or may never acquire especial prominence. This pain is, at first, not aggravated by pressure, and it is probable that the tenderness so often observed later may usually be attributed to peritonitis. About the period of development of pain, or sometimes even earlier than this, the patient realizes that he is not well. Vague disturbances of his digestive organs, anorexia, even disgust for food, and other signs of gastric indigestion, will appear with eructations, flatulence, etc. Vomiting may occur at this time, but more commonly when the disease is advanced, and is then often a result of peritonitis. At other times it follows primary cardiac or pyloric gastric cancer, of which the peritoneal growths are secondary results. Constipation will gradually become persistent and the patient will lose strength and flesh. At this time there will be no fever except in the more acute cases, and the thoracic organs will be unaffected, unless involved in a general carcinosis or subject to independent disease. In acute miliary carcinosis fever may be present from the first and the case closely resemble one of acute miliary tuberculosis.

Sooner or later the belly will become enlarged from the growth of the tumors, from meteorism, from ascites, or a combination of these conditions. In a large number of cases the tumors may be obscurely felt as nodules, varying in size from that of a nut to that of a child's head, deeply in the abdominal wall or more profoundly situated. At times the nodules are replaced by tracts of resistant matter not clearly definable, or by the hardened cancerous omentum crossing the belly as a broad band of induration. When felt, the tumors will usually be hard and resistant, except in the case of colloid cancer, when an obscure sensation of fluctuation may be perceived. Examination by the vagina and rectum will often assist the observer by revealing the infiltrations in the pelvic cavity. Not unfrequently the uterus will be immovably fixed in a mass of such material. The new-formations will often grow rapidly. It is uncommon, however, for them to be easily and definitely recognizable unless of large size, since they are obscured by ascitic fluid and by gas within the intestines. The occurrence of peritonitis will also tend to make a diagnosis difficult by forming adhesions whereby pockets of fluid and knuckles of intestines become fixed in various positions. The surface of the belly will thus be made uneven by the irregular distribution of cancer masses, localized meteorism, and encysted fluid. In the more acute forms it is not at all uncommon for the carcinomatous nodules to entirely escape recognition on account of their small size and wide distribution and the accumulation of ascitic fluid, and the true nature of the disease may not be determined *intra vitam.*

Chuquet claims that a symptom of highest diagnostic importance is the presence throughout the entire subcutaneous system and in the muscles, of "cancer granules" first described by Millard, and which are said to be perceptible to the touch. These "granules," however, do not seem to have been met with by other observers, and can therefore hardly be counted upon with confidence. What seems to be a somewhat similar condition has been described by Chvostek as a scattered crepitation over the belly in peritoneal cancer, or even in peritonitis, quite like skin emphysema. He attributed the sign to fluid enclosed in very small spaces with delicate and sharply limited walls, and which is forced out by pressure. He found it only where the abdominal viscera were adherent to the anterior belly-wall by peritonitic products containing fluid in very small cavities.[1]

Ascites is constantly present in these cases. It is due to peritonitis or to pressure exerted upon venous trunks within the belly cavity, or to hydræmia. It may vary in amount from one to twenty or thirty pints. The fluid is usually clear and of high specific gravity, with floating shreds of fibrin. Its character will often be ascertained through paracentesis performed for the relief of pain or for purposes of diagnosis. It will then be found, very often, to be tinged with blood or decidedly sanguinolent. This condition of the ascitic fluid is of the highest diagnostic importance, and has been insisted upon by a number of writers as indicating a strong probability of a cancerous origin. It must be admitted, however, that it is *possible* for this fluid to be sanguinolent in tubercular and even in chronic peritonitis. All things considered, sanguinolent ascitic fluid gives a strong presumption in favor of peritoneal cancer; and if the sedimentary deposit of this fluid be microscopically examined (a procedure first recommended by Foulis, of Edinburgh), the detection of groups of ordinary epithelial cells will serve to determine its cancerous origin.[1] The amount of fluid is not always proportionate to the duration of the affection. It is often irregularly distributed, by reason of the frequent peritoneal adhesions, which also bind the intestines in such a manner that the usual position of these in simple ascites is not often observed. Another rare peculiarity of the ascitic fluid of carcinoma has been observed by Quinecke, Klebs, and Brieger. Here the fluid is milky white, and forms a creamy surface layer of fat-corpuscles and granular fat. Such cases have been supposed to represent chylous ascites. Brieger, however, thinks that there is no escape of lymph, but that the appearances are due to fatty degeneration of peritoneal epithelium. After paracentesis, peritoneal friction sounds may sometimes be heard. General dropsy frequently occurs later, though œdema of the lower extremities has been known to precede ascites. Pericardial and pleural effusions may arise from cancerous metastasis or extension, and the abdominal wall may become œdematous. Œdema of the lower extremities from pressure upon large intra-abdominal veins may become very intense, and may lead to erythematous, erysipelatous, or even gangrenous inflammation. When pressure is exerted upon the ascending vena cava, the veins of the abdominal wall become very large and tortuous, with reversed blood-current.

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[1] *Œsterreich. Zeitsch. f. prakt. Heilk.*, 39, 1866.

It has been said that constipation is nearly always present early in the disease. This will be associated with more or less localized meteorism. These accumulations of gas sometimes press the diaphragm above its natural limits, and add greatly to the general discomfort. Attacks of diarrhœa may alternate with constipation, and toward the end diarrhœa may become persistent. On the other hand, the lumen of the bowel may ultimately be destroyed, and the patient perish with the symptoms of obstruction. Hepatic and urinary disorders, as direct results of the carcinoma, are not constant. Jaundice may be occasioned by pressure of the new-growths upon the bile-ducts. The renal functions are not often disturbed. The urine will often be highly acid and deposit urates freely. Micturition may sometimes be painful and frequent from implication of the bladder.

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[1] Beatson, *Glasgow Med. Jour.*, 12, 1879; Chuquet, *loc. cit.;* Brieger, *Charité-Annalen* viii, 1883, 109.

While these symptoms are developing, the patient's general condition becomes markedly worse. In a short time, possibly several weeks, more often several months, the various disturbances of nutrition, together with the rapid increase in number and size of the cancerous growths, will have induced that peculiar condition known as the cancerous cachexia, which will, of itself, often direct attention to the true nature of the disease in obscure cases. Tenderness to pressure will be superadded to the ever-increasing distress. Rapid emaciation and increasing debility result from diminished assimilation, pain, sleeplessnes, and general discomfort. The inguinal glands may become enlarged and indurated, and metastatic deposits may occur in other parts of the body. Cancerous infection of the tract of puncture of paracentesis has been observed by Brieger, Reinke, Quincke, Unverricht, and Chuquet. Fever, which may have been present from a very early period, will sooner or later appear and become constant. Pain, insomnia, nausea and vomiting, alternating constipation and diarrhœa, profound debility combine to intensify the sufferings of the patient until death occurs, from the development of the cachexia, from pulmonary œdema, pneumonia, perforation of the bowels, peritonitis, hemorrhage, or some intercurrent affection.

When the peritoneal carcinoma originates through continuity or metastasis, its symptoms will blend with those of the primary affection, and will often play a *rôle* quite unimportant ; or it may become diffused with great rapidity. In such cases, of course, the symptoms of the primary disorder will not be mitigated by the involvement of the peritoneum, but may be less sharply defined than when uncomplicated with those of secondary formations.

Duration and Prognosis.—Peritoneal carcinoma probably runs its course more rapidly than any other form of cancer, ending fatally in from four to six months. Rarely cases terminate within a few weeks after the *apparent* beginning of the malady. On the other hand, life may be prolonged for a year or eighteen months. Vidal reported a case that lasted two and a half years (Chuquet). This seems to be the extreme. Petrina gives the medium duration of primary peritoneal carcinoma, reckoning from the earliest fever, as six weeks, and of secondary cancer as from one to three months. The shortest course observed by him in primary carcinoma was one week ; the longest, six months ; and in secondary cancer the shortest duration was three weeks ; the longest, eighteen months. Other authorities give a longer average duration to the disease, though all agree that the fatal termination will generally come within six months.

Etiology.—This is very obscure. Traumatism has been ascribed as an occasional cause of primary carcinoma of the peritoneum, but this is very doubtful. Probably, also, chronic peritonitis rather occurs as a consequence than as a cause of the new-formation. Heredity exerts a positive but indeterminate influence. Although there seems to be no time of life at which peritoneal cancer may not occur, it is prone to appear after middle life. It has been observed in infancy and even in the fœtus. The following tables of

Petrina and Chuquet show the ages of the patients recorded by them.

PETRINA.			CHUQUET.	
Age.	*Male.*	*Female.*	*Cases.*	*Total.*
10–20	—	1	1	2
20–30	1	2	9	12
30–40	—	2	5	7
40–50	6	2	4	12
50–60	4	7	13	24
60–70	3	7	4	14
70–80	2	3	5	10
	16	24	41	81

It thus appears that nearly one third of all cases occurred between the ages of fifty and sixty, and three fourths of all after the fortieth year. Sex has a decided influence, females having a much greater liability. Of Petrina's forty cases, sixteen were males and twenty-four females. Chuquet records the sex of forty-four cases, of whom seventeen were males and twenty-seven females. The greater liability of females is probably due to the comparative frequency of cancer of the female genital organs, and its extension to the peritoneum. Secondary cancer arises by extension or metastasis from other parts or organs. Chuquet concludes that the centres most often the seat of the primary deposits, are, in order of frequency, ovaries, stomach, liver, and uterus. Petrina's tables show, however, the stomach and pylorus to be the most frequent primary centres of twenty-nine cases of secondary peritoneal cancer, the stomach and pylorus were primarily affected in seventeen, the ovaries, Fallopian tubes, and uterus in four, the liver in three, the pancreas in two, the mesenteric and retroperitoneal glands in two.

Diagnosis.—This is frequently of extreme difficulty, especially in primary carcimoma ; indeed in rapidly fatal cases the nature of the disease may be only determinable after death. At best, the earlier stages usually escape recognition. The affections most apt to be confounded with peritoneal cancer are peritoneal tuberculosis, cancerous and other tumors of the abdominal viscera, ascites from cirrhosis, hydatid and ovarian cysts, meteorism and impacted fæces. When the malady is secondary it is not usually difficult to attribute the new symptoms to their true cause. Their appearance, more or less complete, in a person known to have gastric, ovarian, hepatic, or other variety of cancer, will readily be recognized as indicating an extension or metastasis. Cancer secondary to undetected primary growths will not differ from primary forms in the difficulties of its recognition. Tubercular peritonitis may present the closest clinical analogies with rapid peritoneal carcinoma. There may be the same early development of ascites, the rapid wasting and debility, the abdominal pain, even the formation of tubercular masses detectable through the abdominal wall. Diffusion of nodules to other serous membranes may occur in both affections. Carcinoma, however, may be, indeed, is often quite devoid of fever, which, when present, is quite irregular and unlike the fever of tuberculosis, the evening exacerbations of which are characteristic. The ascites of cancer is more abundant than that of tuberculosis, though in both affections the fluid may be irregularly encysted through inflammatory adhesions. Although pain is present in tuberculosis, it is altogether less than in cancer where it seems often quite out of proportion to the amount of appreciable disease. The existence of tubercle in other organs and the presence of persistent diarrhœa, should give a prejudice in favor of similar disease of the peritoneum. This, however, should not be held too rigidly. Cancerous tumors of the stomach and liver are usually unaccompanied by ascites, unless the peritoneum becomes involved, or secondary deposits occur, compressing the vena portæ or ascending vena cava. In the latter case œdema of the lower extremities will also be present. Hepatic cancer will also show the influence of the diaphragm in following the respiratory movements. Sarcomatous tumors of the peritoneum or intra-abdominal organs, or of the retro-peritoneal glands, do not afford well marked clinical differences with carcinoma. Simple tumors are to be distinguished by their benign course. Ascites from hepatic cirrhosis is of more chronic course, is more painless, and presents altogether a different clinical history.

The apparent cachexia of cirrhotic patients may at times be misleading; but the symptoms of hepatic contraction will be present, and the absence of tumors may be ascertained through paracentesis. Hydatid and ovarian cysts have a slower development and a localized development of fluctuation. They are, also, usually painless, unless complicated with peritonitis. The hydatid thrill, when present, will assist the diagnosis. It should not be forgotten that ovarian cysts are sometimes associated with carcinoma.

Meteorism may distend the belly-wall, but the pure tympanites, and its dispersion under appropriate treatment, will distinguish it. Also, fæcal accumulations may be recognized by their plasticity, and their removal by proper purgation.

The presence of painful intumescence of the belly (with or without fever), with irregularity of surface caused by associated areas of dulness and resonance from fluid, gas, and tumor formations of rapid growth, with emaciation, pain, and the development of cachexia, will usually suffice to distinguish peritoneal carcinoma. Ascites may, however, be so intense as to prevent the recognition of other physical conditions, in which case paracentesis may be resorted to. This should be done with due caution, as it is sometimes followed by unhappy results, especially in cases of colloid cancer (Faucon: *Jour. des sci. méd. de Lille*, iii., 177). The operation is contra-indicated where the physical conditions allow a tolerably certain diagnosis to be made, and should never be done unless the patient be confined to the bed and vigilantly cared for for several days afterward. If, after paracentesis, a solid tumor be detected, the presumption in favor of its malignant nature is strong. The character of the ascitic fluid is of the utmost importance. It may be like ordinary ascitic fluid, but it is very often sanguinolent, a condition almost characteristic of malignant disease. The detection of epithelial cells, such as occur in carcinomatous growths, in the sediment of this fluid, may be considered as conclusive of its cancerous origin.

Pathological Anatomy.—In necropsies, after the more acute primary cancers of the peritoneum, the nodules may be found scattered over every part of the membrane. They may be no larger than millet-seed, and of a whitish or grayish color, and without inflammatory areola. Miliary tuberculosis may be closely simulated. Usually, the nodules vary within much wider limits, often attaining the size of hen's eggs or oranges. They remain sharply circumscribed, or merge into masses of greater or less extent, sometimes forming great plates of infiltration. In the mesentery, Chuquet asserts that the nodules commonly are situated near the points of intestinal attachment. Nodules of the size of a grain of wheat, or of a pea or bean, are very apt to have central depressions, differing thus from tubercle. In the process of growth they tend to assume a spherical form, and may even become pedunculated, showing a resemblance to clusters of white currants (Bristowe); or the pedicles may become reduced to mere threads, and indeed, as in a case reported by Matthews Duncan (*Med. T. and G.*, 1872, ii., 432), they may be severed and the nodules float free in the abdominal cavity. Sometimes the new-growth at first looks as though drops of melted white wax had fallen upon the membrane and there hardened, an appearance also met with in cancer of the intestinal mucous membrane. When diffused infiltration of the membrane occurs it may, in proportion to the rapidity of its development, have the characters of medullary or of scirrhous cancer. The colloid change may affect either variety.

In medullary cancer, soft, brain-like masses may line the parietal peritoneum forming infiltrations of great thickness; or, involving the peritoneal coat of viscera, may deeply imbed them, sometimes without involving their structure. The greater omentum may thus become greatly thickened. Such masses may form, with inflammatory products, conglomerations of bowels, peritoneum, and viscera. This is especially the case in the pelvic cavity. The new-growths will be of light or dark gray or reddish color, with often brownish centres from extravasation. Scattered throughout the masses little cystic accumulations of clear gummy fluid may often be discovered. A granular or mammillated aspect will be presented, while in structure such friability may be presented that the infiltrations readily break down under the finger. Masses may attain the appearance and size of the adult human brain.

In more chronic primary carcinoma, the new-growth may be more localized and the tumors be much fewer in number, though sometimes reaching great size and weighing several pounds. The softer varieties are usually very vascular. In the scirrhous forms, the peritoneum may be thickened by a dense infiltration, giving it a leathery appearance; and the great omentum, reduced to a contracted and thickened band, will stretch across the abdominal cavity. Colloid cancer, which will be frequently met, will be recognized by its semi-fluid, gelatinous appearance.

It is apt to form great masses, and to be widely diffused. In all varieties the peritoneum will often be opaque and thickened almost throughout. Primary peritoneal cancer may also be metastatic, attacking neighboring parts or viscera; or, it may spread by continuity, growing into adjacent organs from the surface, or, extending through the diaphragm, involve the pleuræ. Sometimes organs are completely destroyed by the infiltrations. This is especially the case with the ovaries.

Secondary cancer of the peritoneum will usually be found most densely distributed near the primary centre, whence it has extended by continuity. In such cases, the colloid form is very frequently observed. The anatomical conditions will only differ from those of the primary forms in the superadded pathological changes. The intestines will show various degrees of alteration. They will be more or less constricted, the narrowing reaching its highest grades in the cases where the mucous membrane was primarily affected. Swelling and œdema of the mucous membrane are often found. In all cases evidences of peritonitis will rarely be absent; and masses will sometimes exist where central softening has occurred. Perforation of the bowels, with escape of their contents into cavities thus formed, is not unknown; and limited areas of fatty degeneration and calcification occasionally are detected. Evidences of hemorrhages, of greater or less extent, are not unfrequent.

Treatment.—As the affection runs a necessarily fatal course, treatment must be directed toward the alleviation of suffering and the assistance of the powers of assimilation. Other indications for treatment must be met as they arrive. No treatment addressed to the carcinoma itself will avail.

A CASE OF ACUTE POLIOMYELITIS IN THE ADULT.

By JOHN VAN DUYN, M.D.,

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History: F. D., a clothier's clerk, twenty-three years old, of average height and good muscular development. Had worked on a farm during summers until 1880, which year he spent at school. Early in 1881 he entered a clothing-store as clerk, but in the following November he had malarial fever, and in February. 1882, had a moderately severe attack of typhoid fever. From the time of his recovery, in early spring, until the following October, he worked on a farm, and then resumed his position as clerk. Had never had venereal disease, and had never been intemperate in the use of alcohol.

Aug. 24, 1883, he walked nine miles, arriving at his destination at two o'clock the next morning, went to bed at three o'clock, and slept six hours. At noon, he engaged with others in "putting" a stone, which weighed nearly forty pounds, and in jumping. The stone was "put" from the shoulder by the right arm forward, and thrown by both arms over the head backward. This contest was most violent and lasted two hours. Then he ate a hearty dinner. At 4.30 P.M. he walked over three miles, then rode in a stage two miles, and in the cars ten miles, after which he walked with friends in the city, and finally went to bed at eleven o'clock. The next day he felt some soreness, which, in a day or two, entirely passed away. Until the end of the two weeks following, his life was as it had been before. By his exertion he did not become overheated, was not chilly, nor did he take cold. On September 7th, when he woke in the morning, he felt severe pain between the shoulders, equal in intensity on both sides of the spinal column, which was greatly increased by turning the head to either side. He went to work, but during the day he felt chilly and his face was very red. In the afternoon the right hand began to lose its grasp, so that he could hardly hold the pencil in making sale-tickets. The power to use this hand grew gradually less, until, three days later, it disappeared entirely. That evening he rode ten miles by rail. He could not sleep because of headache, chills, and nausea. The soreness became universal. The second day, he got up, could raise the right hand to his face, but could not button his clothes with it. He walked to see the village doctor, but found the strength of his legs failing to the extent that, in the afternoon, when he rode home, three miles distant, he could with difficulty get into, and out of the wagon. Went to bed at night and "took a sweat." The third day, felt sick and sore, but could get up and even went out of the house. When he tried to put on his hat with his left hand, the right being powerless, he found it impossible, and with difficulty only could he lift this hand to the latch of the door. After he had gone to bed at night he found he could not raise either elbow from the bed. From this time on he was confined to the bed. He suffered from general soreness. To breathe gave him pain, and he could not lie on either side, because of the pain which the pressure on the chest gave him. The whole body, excepting the head and neck, was so sensitive that suffering was caused by the gentlest touch. During the following three or four days paralysis of motion became general in the extremities, but not affecting the head, neck, thorax, and the left forearm and hand. Could not raise his heels from the bed, but while on his side, could partially flex the thighs on the abdomen. Constipation became severe, but the bladder was unaffected. During this time the temperature was twice observed and recorded. On the fourth day it rose to 102°, a few days later to 101°. After the tenth day improvement began to be manifest, and continued until the end of the second week, when he could get into a chair, but could not stand, nor could he raise his feet from the floor. The soreness had receded to the shoulders. At the end of the third week, could walk with assistance, but could not lift the feet higher than enough to prevent dragging. Constipation continued and did not disappear until two months later. The right arm was powerless. The left arm was recovering strength rapidly. This arm had not been affected below the elbow. At the end of the fourth week he could walk alone, the right leg having regained considerable strength, but the left leg could as yet be moved onlv with difficulty. At this time he began to feed himself again, by using the left hand after the elbow had been rested on the table. Throughout there had been no abnormal sensations, no numbness. During these four weeks he lost sixty pounds in weight.

Examination, Jan. 12, 1884: Complete loss of voluntary motion in, and great atrophy of all the muscles supplied by the right brachial plexus, excepting those supplied by the fifth and sixth cervical roots. The right hand, with extreme atrophy in all parts, was *en griffe*. The muscles of this extremity, including those of the shoulder, were unaffected by faradism, but all, with the exception of those in the hand, reacted to strong galvanism. On the left side the supra-spinatus and infra-spinatus were greatly atrophied, the pectoral muscles less so, while all contracted under faradism. The elbow could with greatest difficulty be raised to the level of the shoulder. Below the shoulder the left arm was normal. Power to raise either knee was diminished. The left anterior tibial was not under control of the will and was atrophied, but reacted to strong faradism. Because of the loss of this muscle the foot falls in extension when raised.

To the present time, May 16th, treatment by electricity has been almost daily. All the muscles of the right upper extremity contract under galvanism, with the formula CCC. > ACC., while the brachialis anticus, biceps, sup. longus, and pectorales major and minor contract under faradism. The pectoral muscles, biceps, and anterior brachial show decided contractions by the will. The nutrition of these muscles under the influence of the will so far exceeds that of the others that their growth causes them to stand out in marked contrast with the surrounding atrophy. The will has control of all the affected muscles of the left shoulder and of the left anterior tibial.

This typical case of acute spinal paralysis is recorded because of the clear history of the progress and retrogression of the paralytic state, and the manifest relationship between the disease and the violent muscular exertion which preceded it. Should the gymnastic violence be conceded as the cause, than which no other is apparent, then interest must attach itself to the length of time which intervened between the cause and the outbreak of the disease, and to the fact that while both extremities, upper and lower, were affected during the fever, the muscles which remained paralyzed and atrophied were those most violently exercised, and the severity of the results and their permanency are in direct ratio with the use of the muscles.

AN UNIQUE CASE OF POLIOMYELITIS ANTERIOR ACUTA OF THE ADULT.

By AMBROSE L. RANNEY, M.D.,

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I DESIRE to call attention to a case which has lately come under my personal notice, and which seems to present some very striking peculiarities. I refer to a marked attack of poliomyelitis of the acute (?) form in the adult, which was followed by a paralysis of the leg and arm of opposite sides. Although in the literature of the subject different forms of paralysis—such as monoplegia, paraplegia, hemiplegia, and paralysis of all four limbs—have been recorded, I am unaware of a case where there has been the peculiar combination of symptoms which I now record. In Seguin's brochure upon this disease, published in 1874, he cites from Duchenne four cases, three of which had all of the limbs affected, while one developed paraplegia. Those selected also from Charcot and Gombault, as well as the personal cases cited by that author, do not contain a single instance of a similar type. In my search through the various text-books and the journals published since that date, I have failed, as yet, to detect a parallel instance. If I am correct in my belief and in my diagnosis, the case presented is unique.

The history of the case is published as it was noted at the time of my clinical examination. I hoped to be able to publish a more detailed statement in regard to the exact measurements of homologous parts, the electro-muscular reactions, etc., but I was disappointed by the patient failing to keep an engagement made with me for that object. I was unable also to obtain a photograph of his limbs for the same reason.

The history obtained from the patient is given in full.

A young man presented himself at one of my clinics in Burlington, Vt. (April 27, 1884) with the following statement of symptoms :

Age twenty years. Unmarried. Occupation—that of a laborer. In the summer of 1882, patient was employed in a saw-mill, and was obliged to run a treadle with his left foot and to push boards against a circular saw with his right hand. Shortly previous to the time of the development of his nervous trouble, he was severely stung by a swarm of bees upon the exposed portions of his body. He noticed no particular results from the accident, however, and the wounds healed kindly. After a day's fishing, the patient slept upon the wet grass and was attacked immediately with a marked febrile disturbance, which was followed by a paralysis of sudden development.[1] The paralysis was confined exclusively to the *left foot and leg*, and the *right upper limb* (the ones which he constantly employed in his daily vocation). The leg-paralysis disappeared with great rapidity, and in a few weeks he was able to use the right and left lower extremities with equal power. The right arm, however, never regained its power, and began to give evidence of rapid wasting of the muscles of the shoulder and hand. He has never noticed a wasting of the muscles of the lower extremity.

On examination, the grasping power of the right arm was almost abolished ; the muscles of the thumb were atrophied to such an extent as to allow the fingers of my hand to perceive each other with distinctness when they grasped the hand between the metacarpal bones of the thumb and the index-finger. The "bird-claw" deformity of the hand was partially developed, but the interossei showed a less-marked wasting than existed in the thenar eminence. The deltoid was almost completely destroyed, and the muscles of the scapula were also affected to a marked degree. The circumference of the limb was less than that of its fellow. A very strong faradic current produced slight contractions of some of the muscles of the hand and forearm, but the entire limb seemed to respond very feebly to the current, and no electro-reaction occurred in the muscles of the thumb. No constant-current battery was at hand; so that the "reaction of degeneration" was not positively demonstrated, although it would undoubtedly have been found in many of the muscles.

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[1] The patient stated at first that the paralysis came on "inside of a week" ; but on being questioned sharply, he stated that "he was not sure but that it occurred in two days." He was positive, however, that it developed not more than seven days after sleeping on the wet ground.

The lower limbs exhibited no atrophic changes or loss of muscular power. The sensibility to tactile sensations and the impressions of pain and temperature were normal in all of the limbs. A careful examination was made of these points. The patient had been subjected to massage and the faradic current at intervals for months, but without apparent benefit. The bladder and rectum had never given any evidences of impairment.

Certain peculiarities in this case struck me, as follows:

1st. The continued use of the right hand and left leg seemed to act, to some extent, as an etiological factor in the case. The cord was apparently attacked in those segments that had been subjected to excessive and continuous use, and the lateral half of the cord had been involved in each instance. The left arm and right leg, which were not employed in his occupation, were not attacked.

2d. The leg-paralysis recovered quickly and perfectly; but the arm and hand never regained their power. He had been severely stung upon the arm but not upon the leg, although this may not have been more than a coincidence.

3d. The cells of the anterior horns in the cervical segment degenerated rapidly, as evidenced by the rapid atrophy of the shoulder and hand, while those of the lumbar segment experienced no permanent impairment and regained their function in a few weeks.

I can see no elements of doubt as regards the diagnosis of this case. The exposure, the febrile symptoms, the paralysis of sudden advent, the subsequent atrophy of muscles, and the monoplegic type of the palsy, coupled with the fact that the limbs involved were on opposite sides of the body, can be explained to my mind in no other way. The quickly-developed loss of farado-muscular reaction also adds a crowning feature to the list of symptoms. The case seems to be a particularly unique one of this comparatively rare form of disease.

ANALYSIS OF SEVENTY-FIVE CONSECUTIVE CASES OF POSTERIOR SPINAL SCLEROSIS, WITH SPECIAL REFERENCE TO A SYPHILITIC ORIGIN.

BY E. C. SEGUIN, M.D.

IN the last three years a considerable number of observers, European and American, have recorded the statistics of cases of posterior spinal sclerosis, with especial reference to its relations to syphilis. And every addition to these statistics, even if limited in numbers, is help toward the elucidation of the question of the etiology of tabes. Through extensive coöperative work of this kind, we may reap scientific and even practical results of some value. This consideration has led me to go through my case-books and contribute my mite to the subject.

The cases of tabes which have come under my observation since 1866 are naturally divisible into two classes: *i. e.*, those seen in private practice, and those observed at my clinic for diseases of the nervous system at the College of Physicians and Surgeons. The last named cases were examined and published by my friend, Dr. Birdsall, last year,[1] and I do not care now to reproduce that tabular statement. Suffice it to say, that Dr. Fuller, one of my clinical assistants, went to the trouble of going over the clinic case-books again, and reached the same numerical results as Dr. Birdsall.

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[1] "Proceedings of the American Neurological Association," meeting of 1883, *Journal of Nervous and Mental Diseases*, July, 1883.

I now propose to present the results of an analysis of the cases of tabes which have been seen by me in private practice.[1]

The total number of cases was 75, including a number in the first stage, and a few which, while exhibiting many of the symptoms of tabes, were not typical.

I am surprised to find that in a large proportion no mention was made of syphilis.

No mention made of syphilis,	21 cases	28 %
Syphilis mentioned	54 "	72 %

It is therefore better to exhibit the further proportionate results in a double aspect.

I. With reference to the total number of cases of tabes recorded:

Syphilis not mentioned in	21 cases	28 %
No chancre or syphilitic symptoms in	15 "	20 %
History of chancre or syphilis in	39 "	52 %
	75 "	100 %

Thus in more than one half of the total number of cases a distinct history of syphilis or of chancre was obtained.

II. In the more correct relation to the number of cases of tabes in which a positive statement was recorded on the point in question:

No syphilis or chancre in	15 cases	27.78 %
History of chancre or syphilis in	39 "	72.22 %
	54 "	100.00 %

This very large percentage is somewhat in accord with the results of Erb, Fournier, Voigt, Rumpf, and others.

For those who still believe in the distinct duality of the syphilitic poison (*i. e.*, in the existence of non-infecting chancres), the following additional summary may not be without interest:

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[1] These statistical results were presented by Dr. Birdsall, on my behalf, to the Am. Neurological Association at its recent meeting in June.

Cases in which chancre alone occurred	23	42.59 %
Cases in which chancre and secondary syphilis occurred,	16	29.63 %
Cases in which there was no history of syphilis.	15	27.18 %
	54	100.00 %

At first sight this exhibit might seem to militate strongly against the syphilitic origin of tabes; but upon further consideration, when one reflects upon the very large proportion of cases of common (*i. e.*, non-systematic) cerebral and spinal lesions, in which we only obtain a history of chancre, followed by years of good health, then by the nervous lesion, it becomes necessary to drop this objection. But even admitting that there are harmless "soft" chancres, we also know that in a large proportion of cases of "hard" chancres, skilful treatment may indefinitely postpone so-called secondary symptoms.

EDITORIAL DEPARTMENT.

REMOVAL OF THE UTERINE APPENDAGES.

A LETTER

By Lawson Tait.

I have written so much on this subject, that I am almost ashamed of putting pen to paper about it again ; but I am so constantly subjected to misrepresentation, that it is difficult to resist the inclination to reply. The article[1] by Dr. William M. Thallon obliges me to ask for some space, for whilst he gracefully credits me with far more than I deserve, he also to some extent misunderstands what I have written, or he has taken my views second-hand.

In the first place, I object altogether to the title of his paper : "The Battey–Tait Operation." The method of applying names of persons to particular proceedings is a constant source of error, and it clearly is so in this instance. I have no objection to be bracketed with my distinguished friend Dr. Battey, but as the principles of our proceedings differ entirely, such a union as is proposed by Dr. Thallon is wholly inconsistent. Dr. Battey proposed the removal of the ovaries for the purpose of bringing on artificially the menopause, for the purpose of influencing certain indefinite reflex symptoms. This proposal I have followed five times, and with such bad results that I have said over and over again in published papers that I should not continue the practice. Dr. Thallon is not justified, therefore, in saying that I have done hundreds of such operations, that Spencer Wells has done only one or two, and Keith one or two. Dr. Thallon is mixing up things that are wholly distinct, and I have protested against this mixture very many times.

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[1] Vide editorial in April number of Archives.

The principle of the operation I advocate is to save life and relieve suffering by the removal of diseased organs, or for such conditions of physical disease as appear, on theoretical grounds, sustained by actual experience, to justify the proceeding. I rarely operate save in the presence of physical signs ; and in the exceptional instances of operations performed for subjective symptoms only, I carefully guard myself by elaborate consultations.

Let me first speak briefly of Dr. Thallon's conclusions, and then I may say a few words on some passages in the text of his paper.

His first conclusion is that "the operation (Battey–Tait) is not justifiable for purely nervous symptoms, independently of clearly-marked evidences of organic ovarian disease." I wholly agree with this, only I say this is a criticism of Battey's operation, and not Tait's.

Dr. Thallon's second conclusion is that "it is not justifiable in cases of uterine disease or hydro-salpinx."

In answer to this I say that my proposal to remove the appendages for uterine myxoma, made almost simultaneously by Hegar and myself, and therefore, on Dr. Thallon's plan, entitled to the name of the Hegar–Tait operation, has been in my hands a brilliant success. Mr. Knowsley Thornton has found it so successful that he says it should always be a preliminary to hysterectomy. I agree with him so far, that I believe if it were so, there would be a very small number of cases left for hysterectomy. The success of this proceeding has been so great, and has received such abundant confirmation, that I cannot imagine how Dr. Thallon gets his conclusion.

As to hydro-salpinx, I have written so much on this point that I can only feel that Dr. Thallon has omitted to consult my writings. As it is wholly impossible to diagnose with accuracy the existence of hydro-salpinx without abdominal section, as it is wholly impossible to be sure of its position, tapping is altogether out of the question. The recommendation of tapping as an alternative for removal of the diseased tube, can come only from one who has no practical acquaintance with the disease.

On this point let me just interpolate a complaint against one sentence of Dr. Thallon's article, in which he says: "Mr. Lawson Tait in especial has been singularly rash in minimizing the risks, and he has probably done a great deal of harm in encouraging men to undertake a so-called 'easy operation,' which too often has ended fatally for the patient."

I protest against this as most unfair. I have never said it was an easy operation. I have always said it is one of the most difficult things in surgery. I have never encouraged any one to do these cases unless he was prepared to do as I have done—give up every thing else to follow them. I have always said, and I say again, that a man whose prospect is to do only five or six cases of this kind in a year has no right to attempt them, and that if he does and they are fatal to the patients, he has done a very wrong thing. I have never minimized the danger, I have constantly emphasized it. I have never said that my own mortality, about one per cent., could be reached by any body save by doing as I do, devoting my life to the work, and operating five, six, or eight times a week.

Dr. Thallon's third conclusion, that the operation of removal of the uterine appendages for the relief of uterine hemorrhage, or the arrest of uterine tumors, I have already answered.

Dr. Thallon's fourth conclusion I accept with more than pleasure, as it is conveyed in words extremely gratifying to me. I have had a hard fight during eight years to establish my position, and now, thanks to Dr. Kingston Fowler, of the Middlesex Hospital, it has been completely established. That my work is accepted as "one of the most glorious achievements of the surgery of our age" is reward enough for all my trouble.

Dr. Thallon raises the much-discussed question as to the influence of removal of the ovaries and tubes in menstruation. Let me say here again what I have recently said completely, and several times incompletely. Removal of the ovaries alone has little or no influence in arresting menstruation. Removal of the tubes alone arrests menstruation in five out of six cases. Removal of both tubes and ovaries has the same result as removal of the tubes. There are some cases in which even removal of the fundus uteri together with both tubes and ovaries exerts no influence whatever on the periodic regularity of the menstrual flow. In the course of a few days a paper will appear in the *Medical Times and Gazette* comprising the doctrine of Ritchie and Reeves Jackson, that menstruation and ovulation are wholly independent functions. Dr. Thallon seems never to have come across even a suggestion of this before, and, therefore, much of his criticism of my own views I cannot answer here for want of time and space. But if gynæcologists for the future will approach this question with the glimmering of an idea that it is just possible the two functions may not be dependent one on the other, much trouble, confusion, and repetition will be saved.

As to the influences of the removal of the uterine appendages for masturbation or nymphomania, I am quite satisfied that the prospect of good being done is very small. I have, therefore, persistently refused to operate in such cases, as I have not found that removal of the appendages has deprived women of their sexual appetite. Only a few weeks ago, at the Harveian Society, Mr. Bryant of Guy's and myself were able to quote cases where supravaginal hysterectony did not in the least interfere with the sexual appetite. Even if removal of the uterine appendages did destroy it, I for one would not hesitate to operate for the cure of disease ; and I would look upon any man as an inhuman brute who would refuse relief to his suffering wife because his lusts would have some smaller gratification. The only men who would take such a view of their position would be those who had brought the troubles of hydro-salpinx and pyo-salpinx on their wives as the result of gonorrhœa. To me it becomes more and more amazing that this disgusting argument should be discussed. It is our duty to save life and prevent suffering, not to pander to the degraded desire of the debauché.

Dr. Thallon points out what is perfectly true, that, whereas, I do a hundred such operations, Sir Spencer Wells does one or two, and he further says that we must see about the same class of cases. I admit this, but I plead that I am a generation younger than Sir Spencer Wells, and I admit that when my time comes I shall probably have the same difficulty in taking up a new theory that Sir Spencer has shown in the present case. Some months ago Dr. Horrocks asked me at the Obstetrical Society how it was I saw so many cases of pyosalpinx and that they saw none at Guy's Hospital. My reply was that there must be large numbers of them at Guy's Hospital but that they were not discovered. Dr. Thallon says that I take particular delight in saying that in many of my cases operation had been previously refused in London, and if Dr. Thallon knew how much I have been misrepresented and maligned by some of my London brethren he would not be surprised. But my delight has been greatly intensified by a paper read last week by Dr. Kingston Fowler, which proves up to the hilt every thing I have said on the subject, and completely covers the ground raised by Dr. Horrocks, and justifies my answer to it. In the discussion on Dr. Fowler's paper, Dr. W. S. Playfair said that a middle course between the practice at Birmingham and that of those who denounced the operation was the safe one. He illustrated this safe course by his own proceedings, which resulted in the awful mortality of twenty-five per cent. All I can say is that I hope I may never fall into this safe course of Dr. Playfair, for if I do I shall speedily give up practice. No man is justified in operating in such a way. What I constantly plead is that these operations can never be justified save by a very low mortality. If my mortality went up to twelve per cent. I should stop.

I entirely endorse what Dr. Thallon says about the cases where the operation is done for "pain, epilepsy, or insanity." It is too much of the nature of a leap in the dark to have my support. I do not condemn it, however, for there have been some brilliant successes, and there ought to be no mortality at all when there is merely a removal of normal appendages. Speaking of a case related by Dr. Fordyce Barker, Dr. Thallon says he thinks I would not have hesitated to operate in it. He is entirely wrong, for as he gives the case, I should certainly have refused, and have done so in several very like it.

Upon two points let me say that I entirely agree with Dr. Thallon. He says: "Where, however, one ovary lies so low down as to be the first thing encountered by the finger after entering the vagina, especially if it is bound down by adhesions, or by a heavy and retroverted uterus, the chances of any thing short of the radical operation effecting a cure are very slight." This is absolutely true, and some of my most brilliant successes have been in precisely such cases.

Again, Dr. Thallon says: "I believe it is, broadly speaking, true that the more extensively the ovaries are matted up in the products of an old peritonitis, the greater will be the necessity for the operation." This is quite correct, but I cannot at all agree with Dr. Thallon when he further states that this "suggests the unpleasant reflection that the greater danger is there of it being fatal."

In conclusion I have only to say that I have examined with great care the contents of many Fallopian cysts and have never seen, though I have specially looked for, columnar ciliated epithelium. The suggestion of Dr. Frank Ferguson has therefore been anticipated and exhausted.

BIRMINGHAM, *May* 6, 1884.

DR. THALLON'S REPLY TO MR. TAIT.

The answer of Mr. Lawson Tait to some points in my article is chiefly interesting in what it admits. I am extremely glad to find that two of my four conclusions are so heartily endorsed by him. I think it will certainly surprise some of the enthusiastic gynæcologists who have been doing "Tait's operation" for all sorts of purely nervous symptoms ("pain, epilepsy, and insanity") to learn that Mr. Tait himself "wholly agrees" with me that the operation is not justifiable for this class of cases,—and that "it is too much of the nature of a leap in the dark to have my (*i. e.* Lawson Tait's) support."

That declaration is decidedly more explicit than any limitation to the operation which I have hitherto been able to find in Mr. Tait's writings in spite of pretty careful search. And I may as well say just here—that my knowledge of Mr. Tait's published writings is in no case second-hand, as he seems to imagine. I think far too highly of their distinguished author, and appreciate their positive and vigorous style too much, to allow any of them to pass me by unread.

Mr. Tait objects to my title of the Battey-Tait operation, because he and Dr. Battey do their respective operations for different reasons. That is quite true, but then the results of an operation are what it is judged by. It is doubtless very self-comforting for Mr. Tait to suggest that all the errors arising from removal of the ovaries are due to the ideas of his "distinguished friend Dr. Battey," while all the gains are due to his own practice of also removing the tubes. But that is not very convincing, and if we accept Mr. Tait's personal experience that he has done Battey's operation "five times and with such bad results" that he will "not continue the practice," I imagine it will be with the logical consequence of making the profession somewhat sceptical of the good results of "Tait's operation." In the meantime I must beg to retain the title I have adopted, if for no other cause than to emphasize the fact that there is so much diversity of reasoning underlying this operation.

The main points on which Mr. Tait and I are at issue concern the value of the procedure in cases of uterine disease, hemorrhage, and myomata. And this incidentally raises the question of the influence of the removal of the ovaries and tubes on menstruation.

Mr. Tait speaks with no uncertain sound on these points. While Dr. Battey proposed his operation of removal of the ovaries for the very purpose of bringing about the cessation of menstruation, Mr. Tait boldly declares that Battey's operation has little or no influence in this direction. On the other hand, he alleges that the Tubes are *fons et origo mali*, and that their removal alone five times out of six will stop the flow? How does he know that? In how many cases has he removed the tubes alone? On page 66 of his book on "Diseases of the Ovaries" (fourth edition, Wm.

Wood & Co., 1883) he says: "I always remove the ovaries along with the tubes, as without the ducts the glands are of course useless." Again, on page 327, in discussing this class of cases, he says: "In my own practice the conclusion is indicated that removal of the Fallopian tubes is more important than removal of the ovaries, and in by far the larger number of my cases that alone might have sufficed—indeed, in many it has done so." With the physiology underlying this practice I entirely dissent, and submit that it is radically unsound. Mr. Tait's main addition to our knowledge seems to me to consist in showing the only method of relieving a large class of sufferers from otherwise incurable salpingitis, but the incurable conditions in these cases are in nowise due to the functional importance of the tubes, but to their anatomical situation and conformation. In support of the very important functional rôle he ascribes to the tubes he has adduced no evidence. In his belief he apparently stands alone, for even though menstruation and ovulation were proven to be entirely distinct and independent functions—which is very far from being the case,—he would still have to bring forward any evidence that the tubes are the main factors in causing menstruation. The energy of Mr. Tait's statements on this whole question cannot obscure the fact that the whole burden of proof lies with him. He seems to me in this portion of his reply to be pleading like an advocate at the bar in defence of Tait's operation. That is a false mental attitude. I think on such an issue partisanship is not likely to carry much weight in overthrowing long-established scientific beliefs, and the impartial seeker for truth is tempted to exclaim, as a famous Englishman did after listening to a famous and dogmatic colleague: "I wish I was as cock-sure of any thing as 'he' is of every thing." The reasons I brought forward for holding the beneficial effects of the Battey-Tait operation as "not proven" for the cure of uterine myomata and hemorrhage, Mr. Tait makes no attempt to answer, and I need not therefore repeat them.

In discussing the small influence of the operation on the sexua appetite, I am glad to find that Mr. Tait and I so closely agree.

With regard to one point, Mr. Tait complains that I have done him an injustice in saying that he minimizes the risks of the operation. Now I was careful to point out that the wonderful results obtained by Mr. Tait went far to justify him personally in operating, but in his influence as a teacher I think he largely justifies my accusation. To go no further than his very reply in reference to my statement that "the more extensively the ovaries are matted up in the products of an old peritonitis, the greater will be the necessity for an operation," Mr. Tait says: "This is quite correct, but I cannot at all agree with Dr. Thallon when he further states that this suggests the unpleasant reflection that the greater danger is there of it being fatal." Now I would like to ask any fair-minded reader if it is unfair or unjust to say that Mr. Tait minimizes the danger of the operation when he says he cannot at all agree that extensive adhesions add to its danger?

With reference to the diagnosis of the contents of Fallopian cysts from the presence of columnar ciliated epithelium in their contents, the suggestion was based by Dr. Ferguson on the examination of a number of cases. The evidence will in due time be published, and in the meantime I can only say if the suggestion was "anticipated" by Mr. Tait, it certainly was not "exhausted" by him!

In conclusion I am glad to find Mr. Tait so entirely agrees with me in my statements regarding the large class of cases in which the operation is positively justifiable. In helping us to recognize this class of cases, especially those where incurable and painful disease of the tubes exists, Mr. Tait has done great and good service. But while willingly paying my tribute of admiration to the brilliancy of his practical results, I beg to enter my most earnest protest against what I esteem the false physiology, in spite of which those successes have been attained.

WILLIAM M. THALLON, M.D.

Brooklyn, *June* 22, 1884.

NEW BOOKS AND INSTRUMENTS.

Studies in Pathological Anatomy. By FRANCIS DELAFIELD, M.D., Professor of Pathology and Practical Medicine, College of Physicians and Surgeons, New York. Vol. II., Part 1.—"Broncho-Pneumonia." Plates i. to xii. New York: Wm. Wood & Co., November, 1883.

The first volume of Dr. Delafield's excellent "Studies" has been completed in 1880, and comprised the subjects of peritonitis, pleurisy, pneumonia, empyema, hydrothorax, bronchitis, tuberculosis, and phthisis; illustrated with ninety-three plates. The work has received its due share of praise at home and abroad as one of the best productions of American pathology.

The fasciculus before us is a continuation of the above "Studies" and forms the first part of the second volume. The text consists of fifteen royal octavo pages, in large type, giving a concise description of the lesions comprised under the term *broncho-pneumonia*, and is illustrated by seven full-page photographs and five double-page lithographic plates; all being reproductions of original photographs, and drawings from the author's skilled hand.

These illustrations are quite beautiful and well selected; they give the work an exceptional value, and form its essential feature. Strictly speaking the work is an atlas of high artistic and scientific value, for advanced students and specialists, rather than a treatise of pathological anatomy. Some of the drawings are executed under enormous amplification (x 1,500, etc.) which, although quite instructive in giving the extreme histological details, are hardly accessible to the ordinary medical mortal, and less useful than the inviting pictures representing a moderate amplification, such as any student can see and compare with the specimen under the microscope. Fortunately, however the majority of Dr. Delafield's illustrations are of the latter class, and in proportion there are more of them in the fasciculus before us than in the first volume. An admirable feature are the author's photographs of the gross appearances or of pictures only slightly amplified. These are surely true to nature, are useful in conveying to the reader the exact and correct idea of the lesion under consideration.

The twelve illustrations referred to and the accompanying text give us, as stated, a very good and clear chapter of the author's views "on some of the forms of non-tubercular broncho-pneumonia."

Dr. Delafield appears to be an adherer to Virchow's view on the dual origin of phthisis ; he strictly distinguishes a non-tubercular from a tubercular form of broncho-pneumonia or phthisis. Admitting the difficulty in separating broncho-pneumonia from phthisis, he expresses his position and view on this question as follows :

" In both we find the same combination of peri-bronchitis and diffuse pneumonia ; the peri-bronchitic pneumonia characterized by the implication of the walls of the vesicles and the presence of permanent tissue in their cavities.

" In both the cellular infiltration of the walls of the bronchi may lead to their dilatation and to the formation of cavities.

" In both there are inflammatory changes involving the connective-tissue framework of the lung ; and when these changes have once commenced, there is the same tendency to a persistence and extension of the inflammatory process.

" In both we may have much the same clinical histories.

" But in broncho-pneumonia there are no tubercle tissue, no tubercle bacilli, no obliteration of blood-vessels, no areas of coagulation necrosis, and comparatively little cheesy degeneration of inflammatory products."

From croupous pneumonia the author distinguishes the lesion under consideration, thus :

In broncho-pneumonia " there is not merely an accumulation of inflammatory products in the cavities of the bronchi and air-vesicles, as in other forms of pneumonia, but also inflammatory changes in the walls of the bronchi and vesicles, so that the tissue of the lung is much more seriously changed, and changed in a way much more likely to remain permanent."

The author believes that in every pneumonia there is some bronchitis present : if this bronchitis is more prominent than the changes in the pulmonary tissue, or if the latter is affected only in limited areas around the bronchials, the lesion is then to be designated a *broncho-pneumonia.* The characteristic gross appearances of sections of such lungs are well shown in the author's photographs. Grayish peri-bronchitic nodules of various sizes and figures, often densely packed, are seen scattered throughout the parenchyma of the lungs, which otherwise may be merely congested or may be variously affected. Dr. Delafield states that the extension of the inflammation, from the affected bronchioles, is not to the air-vesicles which empty into that bronchus, but into those which are in contact with it. It is "as if a red-hot needle were thrust through the lung, making a tract of charred tissue around it. So each of these inflamed bronchi along its whole length is surrounded by a zone of inflamed air-vesicles."

The author distinguishes an acute form of broncho-pneumonia from a chronic or "persistent" form of this lesion. He describes and illustrates each form by well-studied typical cases observed among adults as well as children, but preëminently in the latter. Each case is accompanied by an autopsy record, microscopical studies, and instructive drawings or photographs of the lung lesions.

The acute form of broncho-pneumonia is identified with "capillary bronchitis" of the adult and the pneumonia of children which follows whooping-cough, measles, scarlatina, diphtheria, etc., but may also occur as an idiopathic affection.

Death may ensue within a few hours from the onset of the attack, or may last days or weeks. But recovery appears to occur quite commonly and perfectly in this affection.

Less frequently, the author says, a chronic inflammation of the interstitial tissue of the lung is developed and the child goes on to have a *persistent broncho-pneumonia.*

The attacks may repeat themselves year after year while the child grows up to adult life. Lungs gradually become consolidated, pleuræ thickened, never returning to normal condition, and "*the patient develops acute general miliary tuberculosis.*"

In all cases of persistent broncho-pneumonia the general conditions are given by the author as follows .

"(1) A chronic bronchitis with or without dilatation of the bronchi ; (2) the formation of new connective tissue in the walls of the bronchi and the air-vesicles, along the course of the blood-vessels, in the interlobular septa, and pulmonary pleuræ ; (3) obliteration of air-vesicles, and the production of epithelium and pus within them. The exact condition in each case depends upon the extent of lung involved."

The author lays further stress upon the emphysematous conditions and collapse of groups of air-vesicles or parts of lungs and upon occasional tuberculous inflammation of the bronchial glands as concurrent affections.

As a whole, the author's descriptions are clear, precise, and to the point. None but favorable comments can be justly made.

It is easy to recognize *acute* broncho-pneumonia as an independent affection. This affection in its typical form is, however, seldom seen upon the autopsy table, as the patients in a great majority of cases either get well or the disease has run a chronic course.

A closely analogous lesion, the induced or inhalation broncho-pneumonia, has of late repeatedly been studied in dogs and other animals and has thrown a good deal of light upon the lesion under consideration. If animals are forced to inhale irritative particles (sputum, pus, or powders of any kind) suspended in the air by means of a spray apparatus, or if the material is introduced through an opening in the trachea in the neck, an exquisite acute broncho-pneumonia ensues in every case. Similar results are often obtained also after paralysis of the epiglottis induced by section of both vagi in the neck, the saliva and particles of food being thus allowed to enter into the unprotected trachea and lungs.

Except in the latter case, the animals as a rule recover within the second month after the operation (even after sputum inhalations), and if an autopsy is made of an animal killed after the lapse of two months the lungs are seen perfectly normal. When, however, the animal is examined within the first month there is seen an *acute* broncho-pneumonia not distinguishable from that described in man by Dr. Delafield. Throughout the parenchyma of the lungs are seen small nodes, of irregular sizes and outlines, resembling tubercles.

Under the microscope these nodules show to consist of mere unorganized collection of cells, often in a state of retrograde change and mixed with mucus and débris ; this exudate being limited to the lumina of the bronchials and their pertaining groups of air-vesicles (the acini) which they fill. On section these artificial boundaries give rise to the appearance of nodes. Very often epithelial cells and mucus from the bronchi have been seen intermingled with the purulent contents within the air-vesicles ; this suggests that the exudate contained in the latter may have been partly aspired from the bronchi (during forced breathing) and was lodged in the air-vesicles as foreign material. This appears to be also the case in acute broncho-pneumonia in man. In fatal cases of acute broncho-pneumonia in animals as well as in man abscesses have been met with.

Chronic broncho-pneumonia, however, must be classed with the tuberculosis lesions in accordance with our present knowledge of what constitutes tubercle ; a knowledge to which Dr. Delafield himself has so much contributed in his "Studies."

From the standpoint of a unitory origin of phthisis it is difficult to conceive a definite distinction between *chronic non-tubercular* and *tubercular broncho-pneumonia*, first on account of the too common concurrence of miliary tuberculosis, and further for other reasons stated by Dr. Delafield himself.

As a really refreshing feature of the author's admirable "Studies" must be mentioned yet his sound and unbiassed reference to the tubercle-bacillus question. [H. F. F.]

Psychiatrie: Klinik der Erkrankungen des Vorderhirnes, begründet auf dessen Bau, Leistungen und Ernährung. Von Dr. THEODOR MEYNERT, Professor der Nervenkrankheiten und Vorstand der Psychiatrischen Klinik in Wien. Erste Hälfte. Wien, 1884. Wilhelm Braumüller.

In an article published in 1878, Professor Meynert incidentally refers to his treatise on psychiatry which he was then preparing for the press. More than six years have since elapsed, and the first half of that work has but just been presented to the medical public. The delay in its publication can not be attributed solely to the author's avowed dislike of book-making; in part it is undoubtedly due to the careful manner in which Prof. Meynert has sifted his own thoughts, and the facts furnished by others, before putting them to print. Having a distinct message to impart, he knew that, whether published a year or two earlier or later, the originality and worth of his views would not be disputed by others.

To all medical men, and particularly to those interested in the study of the brain, Meynert's name is well known. His anatomical researches rank with those of Clarke, Burdach, and Stilling. Our present knowledge of the relation of the subcortical ganglia to the hemispheres, of the minute anatomy of the cortex, of the tegmentum, and of the projection-systems, is based largely upon the investigations of Meynert. No wonder, that Wernicke, an excellent judge in such matters, should call him "the greatest brain-anatomist that ever lived"!

Of late years Meynert has put his extraordinary knowledge of the structure of the brain to the best possible use—to the study of the physiological and morbid conditions of that organ. At various times and in various pamphlets he has indicated the nature of the problems he has endeavored to solve. This attempt to strike out on a new line of research in the study of the origin and nature of mental diseases lends special importance to the work before us. The title-page tells its story : Mental diseases are declared to be diseases of the fore-brain, due to disorders in the structure, functions, and nutrition of that part of the encephalon. Former authors, and among them the greatest names in this domain of medical science on both sides of the Atlantic, have studied carefully the clinical manifestations of mental disease, but have never ventured an explanation of the various forms of insanity based upon anatomical and physiological data. A recent writer on this subject condemns every effort to lift the curtain from the mysteries of mind, as premature and imprudent. Meynert is of a different opinion. He thinks there is no need of leaving it to posterity to make deductions from anatomical and physiological discoveries which we have made. New facts should be utilized at once. Mistakes are inevitable, but succeeding investigators will profit by them.

In the very first chapter, on the anatomy of the brain, the author shows his thorough mastery of the subject ; in this section those structures are mapped out and thoroughly explained which have an important bearing upon the study of mental diseases. Those who expect this part of the treatise to take the place of a text-book on cerebral anatomy will be sorely disappointed. Among recent authors, Wernicke, Schwalbe, Luys, and Meynert himself (in Stricker's " Handbook of Histology ") have given more detailed accounts of the minute structure of the brain. Instead of burdening the reader's mind with a vast array of facts, Meynert prefers, in this instance, to have him conceive the brain as a unit, and to have him understand the relative arrangement of its parts. Sufficient embryological data are given to show the development of the adult brain from the primary medullary tube, and more especially the growth of the fore-brain from the lateral appendages of the primary anterior vesicle.

Whether we consider the cerebral structure from an anatomical, physiological, or biological point of view, we must, according to Meynert, distinguish between the cortex and the sub-cortical ganglia ; we must remember that the cortex is the one surface upon which all the impressions from the body and the outer world are registered, and that the sub-cortical ganglia are way stations, as it were, through which the peripheral nerve-tracts pass on their journey to the cortex.

The division of these nerve-tracts into projection-systems is too well known to be more than alluded to here. The other (intra-cortical) system of fibres—the association-system—plays an important rôle in the physiological and later sections of this book. Professor Meynert has devoted great care to the proper elucidation of these fibres. Lying entirely within the cortex, and forming closely interlacing groups, they can be most clearly demonstrated by having recourse to the old method of detaching the various bundles of fibres from their proper connections, without creating artificial divisions. Forel has been loudest in his objections to this "detaching" method (Abfaserungsmethode). It may prove a dangerous weapon in the hands of an amateur anatomist, but not when practised by Burdach or Meynert. The author's preparations, of which excellent drawings are given in the book, convince one of the truth of what they are intended to express.

Meynert has deprived cerebral anatomy of much of the mystery which once attended it.

Of great interest in the chapter still under consideration are the brief allusions to the cause of variation of cerebral structure among the different species of animals. Thus he clearly shows that the greater or lesser development of the fore-brain is the controlling factor in shaping the entire cerebral mass; that upon such variations in development depend the relative size of the *pes pedunculi*, of the pons and the pyramidal tracts, and indirectly the appearance or (apparent) disappearance of the *corpus trapezoides* and olivary body in animals and man.

Excellent as this chapter is in all other respects, we regret that it was not revised shortly before the appearance of the first half of this work, for during the last few years Forel, Schwalbe, v. Monakow, and particularly Roller, have added greatly to our knowledge of the minute anatomy of the brain. It is barely possible, however, that Meynert will refer to the conclusions of these investigators in an appendix which he promises to issue with the second half of the volume.

From the chapter succeeding the one just reviewed, the student of psychology, as well as the medical man, may reap a rich harvest. As in the anatomical part, so here the intimate relations between the cortex and the sub-cortical ganglion is forced upon our attention. There is a *functional* relation corresponding in importance to the *anatomical* relation between these parts. The normal functions of the *cortex* are considered first. Once having accepted Bell's law of the centripetal and centrifugal conducting-power of the posterior and anterior nerve-roots respectively, the author shows that we need merely postulate the sensitiveness of the cerebral nerve-cell in order to gain an insight into the cortex as the organ of intelligence. Allowing the cortical surface of the child's brain to be a *tabula rasa*, let us suppose that it (the child) meets for the first time a bleating lamb. Through the various parts of the projection-system two impressions are carried to the cortex : a visual image of the lamb, and an auditory image of the sound it produced. These images, we have good reason to believe, will be stored in different parts of the cortical surface ; as there is no irradiation (practically) in the gray substance of the cortex, the cells, harboring these impressions, would be anatomically and functionally separate but for the presence of the association fibres connecting them. Such an association, once formed, is a permanent one, and one of these images cannot be excited without recalling the memory of the other. The cerebral cortex is practically a storehouse of such visual, auditory, and other images. An association group may comprise any number of distinct images or memories. Thus, as the author puts it, a child that has burned its fingers in a flame will add to the visual memory of the flame and to the memory of the painful tactile sensation, the memory of the reflex effort to withdraw the hand from the flame. Here the memory of muscular innervation comes into play ; and later-on the author shows that voluntary movements of which a higher animal that lacks cerebral hemispheres is incapable, are simply effected by re-exciting the impressions registered in the cortex of the previous innervations of certain groups of muscles from the sub-cortical centres. The sum of all such impressions constitutes what we call intelligence. These impressions are scattered over different parts of the entire brain, and hence intelligence must be said to result from the activity of the entire cortex, and not from any part of it. To the development of logical reasoning from the intensity and frequent excitation of definitely associated groups of impressions we can only allude in passing. The explanation furnished of aggressive and repulsive movements has a more direct bearing upon mental diseases.

These are intimately connected with pleasurable and painful emotions. Relying upon the experiments of Schiff and Goltz, Meynert insists that in the case of the decapitated frog, pinching the leg not only causes reflex movements, but that the greater the irritation the greater the resistance offered in the gray substance of the spinal cord to the conduction of the irritant impulses, and that consciousness, when present, recognizes this inhibition as a painful sensation. Furthermore, it has been shown that repulsive movements are attended by a contraction of the blood-vessels of the spinal-cord segment through which the sensory irritation passes ; this in turn causes what the author calls the dyspnœtic phase of the nutrition of the nervous elements. Aggressive movements, on the other hand, are produced by very slight friction (Goltz's croak experiment), are accompanied by pleasurable sensations, due to the unrestricted passage of nerve force, by a functional hyperæmia, an apnœtic phase of nutrition, and so on. Pushing still farther in this direction, the author shows that an actual external irritant impulse is not needed to start this complicated mechanism, but that the mere memory of a former pain may be sufficient to cause a repulsive movement and all its concomitant states. Let those to whom these views appear fanciful look up the full arguments in the book itself before passing judgment upon them. This chapter and the following, on the nutrition of the brain, are full of interesting suggestions to which even numerous extracts would do but scant justice.

In the short chapter on the clinical manifestations of prosencephalic disease, the first practical application is made of anatomical and physiological principles as laid down in the preceding sections of this volume. A few difficulties meeting us at the outset are frankly stated : First—Symptoms due to sub-cortical irritation may screen cortical *lesion-symptoms* (Ausfallserscheinungen). Secondly—Symptoms equal in severity to true lesion-symptoms may be due to irregularities in the nutrition of an otherwise healthy part.

It will be a surprise to many to find predisposition to mental disease put down among the lowest forms of anatomical disorders. I question very much whether in all cases of mental troubles, and particularly in those with a strong *inherited* tendency to disease, it will be possible to assign an anatomical reason ; but Meynert ascribes the predisposition either to some injury to the head during parturition and consequent malformation of the skull, or to the constitutional structural anomalies attendant upon anæmia and other such causes. Whether successful or not, the attempt to define the exact nature of predisposition to disease is a creditable one, and deserves to be extended to other domains of medical science.

In considering functional disorders we must again discriminate between the cortex and sub-cortical centres. The first effect of a change toward defective nutrition of the cortex is a loss of cortical inhibition ; as soon as this inhibitory force is removed the play of the sub-cortical centres begins ; hallucinations and deliria are the result. *Irritation* of the sub-cortical centres may bring about the same result, while a *weakening* of the sub-cortical functions produces an increase of cortical power. Meynert explains this in the following way : Supposing that general anæmia of the brain has produced exhaustion of the hemispheres and of the sub-cortical vaso-motor centres, then a paretic condition of the arterial muscular walls will ensue, and the result is hyperæmia of the cortex. As long as the condition lasts we get a full complement of maniacal symptoms, viz. : pleasurable mood, an abundance of loosely associated ideas and innumerable motor impulses. By finally causing an arterial hyperæmia in the vaso-motor centre itself, this condition works out its own salvation ; the vessels of the cortext contract normally again, and the maniacal symptoms disappear. If this process (of vaso-motor irritation) is carried to the extreme the contraction of the cerebral vessels will become more intense, and all the symptoms of a melancholic mood will be developed.

From the explanation given of normal cortical functions, it is evident that irritation of the cortex could never produce any thing but abnormalities of thought—insane ideas—and pathological emotions ; but the latter have been shown to be mere intensified perceptions. We have already so far exceeded the limits of a review that we can not now dwell upon the manner in which, as the author shows, insane ideas and pathological emotions react upon each other ; nor do more than quote the opinion of Meynert (not peculiar to him, however,) that insane ideas are never newly formed ; that the insane subject (with defective cortical inhibition) simply reproduces such primitive ideas as a child possesses that has no proper appreciation of the relation of its own individuality to the surrounding world. The exaggerated or otherwise defective notions of the normal child are limited and corrected by experience.

The quintessence of Prof. Meynert's views is embodied in his *natural* system of classification of diseases of the fore-brain. It would be an injustice to the author to criticize or even to discuss this classification until after the publication of the clinical portion of the work. If the second instalment of this book will redeem the promises of the part we have reviewed, Prof. Meynert's treatise will form a landmark in psychiatrical literature.

For the benefit of those who will be puzzled by the labyrinthic sentences of the original, we add that an English translation of the book will soon be published by Messrs. G. P. Putnam's Sons, of this city, with the coöperation of a London firm. [B. S.]

Leçons sur les Maladies Mentales. Par le docteur B. Ball, Professeur à la Faculté de Médecine de Paris. Asselin et Cie. Paris, 1883, pp. 884.

The years 1883–84 will remain remarkable for the appearance of an extraordinary number of treatises and text-books on mental diseases—a flood apparently out of all proportion to the demand, and certainly not justified by any special advance in our knowledge of cerebral physiology or pathology. Besides the work before us, we may mention, as a partial list of these new books or editions, Krafft-Ebing's "Lehrbuch der Psychiatrie," 2d edition, 1883; Spitzka: "Insanity; its Classification, Diagnosis, and Treatment," New York, 1883; Hammond: "A Treatise on Insanity," New York, 1883; Mann: "A Manual of Psychological Medicine," Phila., 1883; Hamilton: "Types of Insanity," New York, 1883; Neumann: "Leitfaden der Psychiatrie," Breslau, 1883; Clouston: "Clinical Lectures on Mental Diseases," London, 1883 (American edition, Phila., 1884); and Meynert: "Klinik der Erkrankungen des Vorderhirns," Bd. I., Wien, 1884. There are, doubtless, others, but this partial list is surely enough to appall the would-be thorough student of psychiatrical literature.

The work before us is, perhaps, the most purely clinical of all; it is scarcely pathological, and not at all psychological. It aims, in beautifully clear, concise, often epigrammatic French, to delineate insanity as the physician will see it. Shall we say that it is even more clinical than a work written by a man brought up in the strict asylum hierarchy would be? This would not be an exaggeration, and would find its explanation and justification in the well-known fact, that Prof. Ball was a master in ordinary clinical medicine before he began treating psychiatry. The scientific alienist may find great gaps in his work, but the practising physician will turn to it with ever-renewing pleasure and profit. The work is divided into forty-five lessons or chapters, and is, in reality, a double book, for, up to chapter xxi., the author describes morbid mental states as classically designated by the French school, while beyond this point he takes up the systematic description of the forms of insanity according to a classification of his own, which is (and the author admits it) based on the same etiological and pathological principles as that of Morel. Ball groups insanities as follows :

1. Vesanic or essential .	Circular insanity, Monomanias.
2. Neuropathic . .	Hysterical, Epileptic, Choreic, etc.
3. Diathetic . . .	Gouty, Rheumatic, Tubercular, Cancerous, Anæmic, etc.
4. Sympathetic . .	Genital, Cardiac, Gastro-intestinal, Pulmonary, etc.
5. Toxic . . .	Alcoholic, Saturnine, Morphinic, etc.
6. Organic, or cerebro-spinal	General paralysis, Aphasia, Acute delirium, Dementia with hemiplegia, etc.
7. Congenital . .	Idiocy, Imbecility, Cretinism.

The author does not attach overmuch value to this classification, but claims for it that it will meet the want of the clinician.

In chapters xvi.–xviii. the causes of insanity are well discussed. The great importance of inheritance, when rightly conceived, as a part of evolution, is broadly and strongly sketched. Other causes are classified as general and exciting. Among these injury to the head takes rank as most important. The efficacy of moral causes, especially that of the struggle for existence in a complex civilized state, acting on predisposed organizations, is admitted. General paralysis may result from injury to the head. In another place (chapter xxviii.) the author agrees with Maudsley in thinking that this disease may be caused by conjugal sexual excesses.

The pseudo-general paralyses from lead, from alcohol, from syphilis, and from pellagra, receive attention.

Alcoholism and its numerous psychic manifestations receive what might at first sight seem an undue share of attention ; it occupies chapters xxx. to xxxiii. But if one considers the extraordinary increase of alcoholic excesses in France (and other parts of Europe) in the last twenty years, and its ever-increasing influence as a direct and indirect factor in insanity and race-deterioration, this is justified. Dipsomania is recognized as a morbid state.

General paralysis occupies chapters xxxv. to xlii., inclusive, and is very well treated.

Dr. Ball's therapy of insanity is not encouraging. He has no optimism, and is evidently much influenced by his faith in the degenerative nature of many forms of insanity. Baths, warm and prolonged, and irritant baths, he favors. Opium and morphia are seldom beneficial. He recommends the bromides in melancholia, in epileptic insanity, and in insanity based upon hysterical states. Hyoscyamia is not mentioned, and chloral is said to be the best of all sedatives.

Seclusion is strongly advocated, either at home (imitating asylum conditions) or in institutions.

Non-restraint finds no favor in our author's eyes. He has evidently not seen for himself how perfectly the system works in Great Britain, Germany, and Switzerland, though a week's absence from Paris would have enabled him to do this. He would mechanically restrain suicides and destructive patients. He justly condemns giving chloral and morphia simply to insure quiescence, and probably overestimates the frequency of this practice in asylums.

The work will well repay study, though it is perhaps too bulky for translation. [E. C. S.]

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Original Articles.

ARE THERE SEPARATE CENTRES FOR LIGHT-, FORM-, AND COLOR-PERCEPTION?

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WASHINGTON.

THAT knowledge of the external world, which comes to us through the medium of the organs of vision, has been broadly divided into three classes: 1, the perception of light or the sense of illumination; 2, the sense of form; and 3, the sense of color.

As the phenomena of each of these classes have pretty clearly circumscribed limits, and can, under ordinary circumstances, be sharply differentiated from each other, they have gradually come to be looked upon in some quarters, as distinct senses; correlated, perhaps, but still essentially independent of each other.

The first to distinctly put forth the idea of a separate existence of white light was Hering, in his theory of the perception of colors. As is well known, this theory rests on the supposed antagonistic action of red and green, and blue and yellow light, and of white light and its opposite, black, on three distinct chemical elements supposed to exist in the retina. Later, Steffan and others embraced the idea, and very recently the views in regard to the separate cerebral centres for each have been formulated and put forth systematically in a brochure by Dr. Hermann Wilbrand.[1]

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[1] "Ophthalmatrische Beiträge zur Diagnostik der Hirnkrankheiten," Weisbaden, J. F. Bergmann, 1884, p. 100.

In this monograph the ground is taken that there are three separate and distinct centres in the brain for the perception of light, form, and color. His theory is ingeniously defended by cleverly constructed diagrams, and an appeal to clinical facts. It has received wide attention among ophthalmologists, and will, probably, on account of its plausibility, meet with an acceptance more or less general. On this account, it has seemed to us important to examine somewhat into its merits, and to see how far it is in consonance with the principles of positive science and clinical observation—for, an acceptable theory on any question involving physics and physiology, must satisfy the demands of both.

It is sought, by this theory, to explain certain phenomena of abnormal vision which have been found associated with affections of the brain. These phenomena are: 1, The absence of the perception of color, the senses of form, and light being intact; 2, the absence of color- and form-perception, the sense of illumination being left; 3, the total abolition of the sense of vision when light-perception is destroyed.

In an attempt to explain these clinical manifestations, the following facts are to be remembered, stating them in an inverse order to the above: 1, that the sense of light is never lost without a concomitant disappearance of the senses of form and color; 2, there is never a loss of the sense of form without an associated loss of the sense of color—with an apparent exception which we shall notice later; 3, the sense of color may be lost, while the senses of form and light are intact.

Wilbrand attempts to explain these facts in the following manner: As the sense of color may be lost while the senses of form and light still remain, he argues that there must be a centre somewhere which presides over this sense, and that this centre must have an independent connection with the retina. For the same reason, the sense of form must also have an independent centre with a connection with the retina, independent of that of the centre for light and color; and, similarly, there must be an independent centre with a separate connection for light-perception.

That is all simple and plausible enough, but in order to explain the facts of the unvarying succession of phenomena as described in the preceding paragraph, it has seemed to him necessary to assume a certain connection between the three fibres connecting the centres with the retina. As the sense of light is never lost without carrying with it a loss of the other senses, it is assumed that there is a fibre going from the retina to the cerebral centre which is common to all three, and as it is supposed that the sense of light comes first in the order of centres, an abolition of this centre must necessarily mean an abolition of the other senses, since all the centres are thus cut off from their connection with the retina. When, however, the centre for color alone is destroyed, or hindered in its function, the connection at the centres of form and light are left intact and these senses are unimpaired. When the centre for form is destroyed, the centre for color being cut off from its connection with the retina, its function is also destroyed, and the centre for light alone remains unimpaired.

But in addition to this fibre common to all three centres the centre for form has one which connects it with the centre for light and the retina, and the centre for light has one which connects it independently with the retina. As a result of this arrangement each separate cell in the centre for color has a single fibre bringing it in communication with the retina, which it shares in common with the centres for form and light; the centre for form has two, one in common with the centre for light and color and one in common with the centre for light alone; the centre for light three, one in common with the centres for form and color, one in common with that for form, and one direct and independent of the other centres. From every percipient point in the retina, therefore, there go three separate fibres to the centre of vision in the brain: one to the centre for light, one to that for form, and one to that for color, with the connections above stated.

Into Dr. Wilbrand's scheme there enters an arrangement of the fibres and cerebral cells with reference to the different parts of the retina; but we shall have nothing to do in this paper with any points unconnected with the idea of three separate centres for light, form, and color.

It cannot be denied that this is an arrangement by which it is *possible* to explain the phenomena in question, but it is certainly not in the spirit of modern science to accept such an explanation without some proof, either positive or analogical, on which to base it. The proof in question can only be found in the application of the laws of wave motion as manifested in the domain of molecular physics, in experimental physiology, and demonstrated pathology. Such proof is as yet wanting. The whole scheme is a figment of the imagination, and is furthermore a species of speculation which, if allowed to become common in ophthalmology, will tend to depose it from the high stand it has taken as the most nearly positive part of the science of medicine. It shows, also, to what lengths we may be led when once we lose our hold on the positive data of science, and no longer rest on the basis of fundamental principles.

So far as the physical part of vision is concerned we know that it has to do with wave motion of ether, and the action of this on the ultimate particles of the matter of which the organ of vision is composed. We can afford here to leave undecided the question of the existence, or not, of fibres going from the retina to the brain. No such continuous structures have yet been demonstrated, but the possibility of a single fibre connecting an ultimate molecule of the retina with one in the brain will not perhaps be denied. It would, however, very much simplify a study of the case if we referred the whole matter to the action of wave motion on a homogeneous structure. If we are to accept Wilbrand's scheme, however, we have *three* distinct fibres connecting every *recipient* element at the retina with every *percipient* element in the brain. That would make sufficiently large draughts upon the physical capabilities of the structures involved, but they are doubly increased if we add the three fibres for the three fundamental colors, as demanded by the Young-Helmholtz theory, making *six* fibres from every element of the retina to every element in the brain.

Such fibres, as every one knows, have not been demonstrated, but it does not seem to have occurred to investigators to question the necessity of their existence, and to seek an explanation more in keeping with the demonstrated laws of wave motion as applied to homogeneous molecular structures. We have endeavored in another place[1] to bring these laws to bear on the elucidation of the question of color-perception.

There is nothing so delicately elective as wave motion in a structure whose molecules are properly adapted in their arrangement to this display of its powers. In such a structure any number of different vibrations will pick their way safely and without the least interference with one another, through long distances.[2] No one, we presume, will contend for a separate fibre in the small wire of the telephone for every distinct note which goes to make up the human voice in speech, and yet the retina and optic nerve, which are highly organized structures, have not more required of them in this particular than the inorganic and (so far as we know) homogeneous substances of which the telephone is made. We have in the organ of vision molecular structures acted on by wave motion, why then, in explanation of the phenomena resulting from this action, should we go beyond the laws which have been clearly demonstrated to apply to similar phenomena in the inorganic world?

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[1] "Theories of Color-perception," *Amer. Jour. Med. Sci.*, July, 1884.

[2] Of course it is not contended that these wave motions are transmitted singly as such, but every variation in wave motion is felt in the resultant effect.

When we come to consider the question of three separate centres in the brain, we have passed, it is true, the bounds of physical science, and have to deal with a physiological function and a psychical phenonenon, but even here, we cannot in our attempts to form a theory legitimately go outside the laws which have been found to govern like phenomena.

Let us reduce the physiological and psychical phenomena of vision to their simplest terms. What are light, color, and form? The difficulty with those treating of the subject heretofore has been, it seems to us, that they have not been able to free their minds from the metaphysical system of thought. The words, light, color, and form, are regarded as *entities*, and as having existences in fact, and to be accounted for as such. It is strange that we should have this remnant of the old school of thought engrafted on the positive system of modern science, and ophthalmology is not the only department of thought that has felt its ill-effects.

The phenomena of vision are but the interpretations of impressions made on the retina and carried thence by means of the optic nerves to the brain. Light is only the translation of wave motion into sensation.[1] How this is effected we do not know. But we do know that when waves of ether of all the lengths which are capable of being perceived act at one and the same time, we have a sensation called *white;* when a limited number or a single wave acts, the sensation is known as *colored.* We know further, *that there is no white light sensation that cannot be resolved into its constituent elements of color-sensation.* In the language of physics, there is no white light that cannot be broken up by the prism into the spectral colors.

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[1] Plato defined light to be "the power that through the eye manifests color." The same idea (which is the true one), is also expressed by Ruskin in his own inimitable way in one of his latest utterances ("The Storm-cloud of the 19th Century," two lectures delivered at the London Institution, 1884). "Light and sound are *sensations* of the animal frame which remain and must remain wholly inexplicable, whatever manner of force, pulse or palpitation may be instrumental in producing them; nor does any such force *become* light or sound

White and color, then, being expressions of the effects of wave motion, differing only in degree, cannot be looked upon as distinct sensations. *Moreover, since the whole must comprehend all its parts, white light must include all colored light.*

To suppose a separate cerebral centre for the two, therefore, seems to us about on a par with the proceeding of the philosopher who cut a large hole in his door to admit the cat and a number of smaller holes to admit the kittens.

The sense of form, as far as it pertains to the organ of vision, is but the expression of the idea of extension as represented by the amount and form of the area of the retina impressed. But to call the idea of form a purely visual sensation is erroneous. It is an expression of the judgment which is based on information received by the brain from other sources besides the eye.

The impression received from the retina must be verified by that given by touch before a correct idea of form can be entertained. Long experience has enabled us to supply from memory this second factor, and in ordinary vision, our judgments are formed from the retinal impressions alone, but it is only a process of education which enables us to do it.

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except in its recontre with an animal. The leaf hears no murmur in the wind to which it waves on the branches, nor can the clay discern the vibration by which it is thrilled into a ruby."

To use again the terms employed in physics, light (comprehending color) is a *qualitative* sensation, form a *quantitative* sensation. Light and color are the judgments formed on the basis of the character and number of the ethereal undulations ; form is the idea based on the extent of the retina affected, together with the knowledge previously or concomitantly obtained through the muscular sense and the sense of touch. As has been stated before these ideas are only the translations of impressions carried to the brain—but is it necessary that there should be a separate centre for their perception ? As these ideas are but states of consciousness we would have as much right to suppose a centre for every one of the other million states of consciousness that happen daily in our lives.

In order to avoid a labyrinth of complications we must assume that there is an overruling power of judgment, which takes cognizance of all impressions received, which combines and discriminates, and finally leads to the condition we denominate consciousness. That there is a certain portion of the brain set apart for the reception of particular kinds of impressions is a plausible induction, and is support by many facts in experimental physiology and in pathology. Thus, it is almost certain that there is a particular part of the cortical substance of the posterior cerebral lobe which is intimately connected with the sense of vision, for when it is destroyed the power of seeing is lost. Such a centre will suffice for the reception of all impressions made on the retina, but when we seek to explain how these impressions are interpreted by the faculties of the mind we pass the boundary of physics and physiology, and enter a region concerning the essential nature of which we are in absolute ignorance.

Up to this point, however, we can legitimately go with our wave motion and molecules, in an attempt to account for the phenomena.

Making an application of these laws of wave motion, then, can we consistently explain the three conventional divisions of the visual function—viz: light-, color-, and form-perception—on the basis of one cerebral centre for all? From our point of view, the task is not at all a difficult one. We have already noted the physical impossibility of separating white and colored light on account of the demonstrated fact that white light includes the colors, and that there is no colored light which is not contained, potentially, in the white.

We will assume, for the sake of illustration, that each individual element in the retina has a corresponding element in the cerebral centre, with which it is connected by a single fibre. When one of these retinal elements vibrates in a certain phase, which is set up, we will say, by the ethereal undulation corresponding to the sensation of red; this vibration is conveyed to the cerebral centre, and the corresponding cerebral cell vibrates in the same phase—resultant sensation, red. The same retinal element vibrates to the phase green—which vibration is repeated in the cerebral element—resultant sensation, green. The retinal element vibrates with the ether wave, producing blue; this, repeated in the cerebral cell, gives rise to the sensation of that color; and so on for yellow and all the other distinguishable colors. Let these ether waves, and all the others corresponding to the colors in the solar spectrum fall upon the element at the same time, and the *resultant* vibration phase (according to well-demonstrated laws of physics) will be different from any, and all combinations short of the whole as they differ from each other; this vibration phase, carried to the brain, is reproduced by the cerebral element; resultant sensation, white.

This explanation is simple, in strict accordance with the known laws of the physics involved, and thus fulfills all the requirements of an acceptable working hypothesis.

The idea of form, so far as it comes from the eye, is simply explained by the sensation resulting from the *number* of the cerebral elements (corresponding to the number of retinal elements impressed) affected, and their relations to each other in space.

We have, therefore, in this scheme, but one set of cerebral elements corresponding to the retinal elements on which the impressions are primarily made.

We have not time nor space to enter into an elaboration of this hypothesis in explanation of all the various phenomena of normal and disordered vision. We shall only notice a few manifestations which the scheme of Wilbrand was constructed to explain. These phenomena, as we have noted at the beginning of this article, are the absence of color-sensation (generally in one half of the retina), the sense of form and light being intact, and the presence of the sense of light when both color- and form-sensation are absent. These phenomena are accounted for by W. on the supposition of a destruction in the first instance of the centre for colors, leaving the other two centres unharmed; and, in the second instance, by a destruction of the centres for both color and form, that for light remaining intact.

Cases of hemi-achromatopsia, or defective color-sense in corresponding halves of each retina, are not uncommon. The defect may be partial or complete—that is, some of the colors may still be properly perceived, or it may be impossible to recognize any. Wilbrand gives the clinical histories of six such cases in more or less detail, and the histories of two other cases of "amnestische Farbenblindheit," in which there existed a want of power to express in words correctly the color sensations. Several other cases have been reported which are not in W.'s list,[1] and among them one by Dr. Eperon in the *Archives d'Ophthalmologie*, for July and

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[1] Shoeler reports eight (*Beit. z. Path. d. Shenerv.* 1884), and Noyes two (*Arch. of Oph.*, vol. XI.).

Aug., 1884, which is of especial interest on account of the very careful and complete examination to which the patient was subjected, and on account of a peculiar derangement of other cerebral functions. In all these cases there was a diminished or abolished chromatic sense alone, or combined with loss of form sense, while the sense of illumination remained. This fact in reference to these cases, is to be noted, however, that in none of them, except that of Eperon, was any photometric examination made. Eperon found, in his case, the sense of illumination reduced to $\frac{1}{9}$, and until such examination in other cases reveals a perfectly normal sense of illumination, we cannot accept it as a fact that in complete hemi-achromatopsia there is no diminution of the light-sense. On the other hand, in some of the cases, it is distinctly stated that it is reduced.

In congenital, so-called, color-blindness we have, as a rule, normal visual acuteness, but it is to be remembered that here we are undoubtedly dealing with errors of judgment and not with a pathological condition of the nerve centres.

When there is a pathological alteration in the cerebral centres, its effect must be felt on its molecular structure—in fact, that is the essence of pathological change. The effect of a pathological change in the molecular structure at the centre for vision would, most naturally, be an impairment of the power of the molecules to respond promptly and readily to the wave motions corresponding to the different colors. It is readily understood how delicately organized must be the molecular structure in order that it respond to such very slight variations in wave motion, and how easy it would be to throw it out of its equilibrium. It can also be easily understood how this change in molecular structure might be such as to prevent it from responding to certain wave lengths, while it would be affected by the force of the vibration movement of the whole spectrum acting at once.

On this simple conception is it possible to explain all the phenomena of hemi-achromatopsia that have, as yet, been observed. Moreover, we have a parallel condition in affections of the choroid and retina in which the retinal structure is no longer normal in its molecular arrangements. In a case of extensive choroidal changes which I recently examined there was perception of light over the whole of the visual field, and in the outer, lower and part of the upper inner field there was color-perception which did not depart very markedly from the normal, but in the lower inner field there was no distinguishing of colors. And with this loss of color-perception there was associated in this part of the field a diminution of the sense of illumination. The white square of the perimeter appeared to her to be much " brighter " in the outer and upper and inner field than in that portion where there was absence of color-sensation. In another case of extensive choroidal disease where the visual power was reduced to counting fingers at two feet, there was an irregularly concentric contraction of the field for white. In this field no colors were distinguished except red, which, however, had a " pinkish tinge," and was only recognized in the outer field near the point of fixation. In both these cases the fellow eye was normal. We, therefore, do not believe that there can be an absence of color-perception from pathological causes without a concomitant lowering of the sense of illumination of a greater or less degree. We can conceive it possible, however, that the molecular disturbance may be so slight as to affect perceptibly one or two undulation phases only, the others remaining unaffected. Under these circumstances the sense of illumination might not be much lowered and form-perception intact.

In reference to Eperon's case, it should be remarked in addition, that there was an impairment of judgment in respect to the memory and power of reproducing words that had been *read*, though those that had been *heard* were more easily repeated. How far this change had affected the judgment of color impressions we have no means of knowing, but it must nevertheless remain a possible factor in the case.

It is a clinical fact, brought out clearly in the cases collected by Wilbrand and others, that when the sense of form is lost the sense of colors is lost also, though the sense of light may remain. No case has yet been reported where the sense of form has been lost while that of light and color remained. Wilbrand accounts for this by supposing that the color fibre going from the retina to the brain passes through the centres for white and form before reaching the centre for color. Therefore, a destruction of the form-centre, must, by cutting off the connection between retina and color-centre, necessarily involve a loss of color-perception.

The concomitant loss of form- and color-perception is much more simply accounted for on the principle of variation in molecular motion. A moderate amount of change would so alter the responsive power of the molecules that they could not act promptly to the waves of the different colors, but yet their position and relation to each other may remain nearly or quite normal, so that when they *are* affected by all vibrations of the spectrum at once the resultant sensation will be projected nearly or quite in a normal manner. When, however, the change is so great as to displace the molecules from their normal position, but not entirely to annul their vibrating power, the vibrations still give rise to a sensation, but the position of the vibrating molecules in their relations to each other is so altered from the normal that they cannot be properly projected by the consciousness, and so no distinct image is perceived, though there may be a general sense of illumination. The condition has its analogue in opacities of the cornea through which ethereal undulations may still pass; but the rays are so scattered that no image can be formed. In order to have a definite sense of form a certain number of molecules in a definite order must be affected. When the sense of illumination is retained under these circumstances it will most likely be reduced very much in power—and no cases have been recorded in which it approached the normal.

In connection with this part of the subject there is a very remarkable statement on pp. 23-24 of Wilbrand's brochure. He says: "A separate irritation of the color-centre, as a physiological experiment, without a participation on the part of the form and light sense, has not yet been demonstrated, but the bright red color sensation we experience when looking toward the sun with the eye-lids closed is explained by a simultaneous excitation of the light and color centres to the exclusion of the form centre. Leber says (Gräfe u. Sæmisch. v. 1042): 'we see not colors but colored pictures, the form element cannot be separated from color-perception and the centre of both must be the same,' but the two above mentioned cases and the experiment with the closed lids show that there must be the clearly defined centre for form and color, though ordinarily in every picture formation all three centres are affected."

It is hard to believe that scientific men are serious when they adduce such opinions in support of an hypothesis for which they claim attention. I would ask these gentlemen how we are to experience the sensation of a form when there is no image pictured on the retina? In the experiment alluded to, light passes through the partially translucent tissues of the lids and reaches the retina—but the rays are so irregularly diffracted from their course that no distinct image can be formed, and there is only a diffuse luminosity, the same as when light passes through frosted glass. Its red color is of course due to the blood in the tissues through which it passes. But to say that under these circumstances we can have no sense of *space* is not true, for when the experiment is made with a candle the position of the candle in the visual field can easily be recognized because there is always one part of the retina where the luminosity is most intense, and this is properly projected. It is to be borne in mind in this connection that the sense of form is only a more refined or highly developed sense of projection.

We would call attention at this place to another physiological experiment which, it seems to us, will be difficult to explain on the three-centres theory. We allude to the so-called after-images or the residual sensations of white light. When white light, either direct from the sun, or reflected from as nearly a white surface as we can find, is thrown into the eyes for a few seconds, and the lids are then closed and the hands placed over them so as to exclude all extraneous light, the after-image undergoes a series of changes in *colors*, beginning usually with blue and ending with red. In some of the hundreds of these experiments that I have made, I have noted as many as seven different colors and shades; but *never* after the eye is closed has there been a sensation of white. Have we here a sensation of color independent of white light? Certainly not. Even if Newton had never made his immortal discovery, this simple experiment would be sufficient to establish the compound nature of white light, and it removes beyond the pale of controversy the question of the composite character of the sensation of white.

If white light is a distinct sensation, and has a centre of its own, its after-images should be not colored, but shades of gray, because, in Wilbrand's scheme, while a color-sensation may include white, the converse is not true, and white does not, of necessity, include colors.

The phenomena in question are easily explained by the theory of the action of wave motion on molecules. White light, holding all the undulations corresponding to all the perceivable colors falls upon the retina and the vibrations are carried to the visual centre. These vibrations vary in intensity and amplitude, according to their corresponding colors, but when they all act *at once*, the sensation is white.[1] When the eye is closed, however, and the retina is cut off from the source of illumination, the vibrations already set up in the visual apparatus still continue in accordance with the law of inertia ; but soon some of them stop and then there is a destruction of the equilibrium of the color-forces, the stronger come to the fore, and there is no longer a resultant sensation of white, but of the predominant color or colors. We have not time to examine here into the reason why blue should be the first color to manifiest itself, but it is probably due to the fact that, the blue waves being more rapid, act more quickly and forcibly at the beginning, but are, on the other hand, soon exhausted, while the red, though less active, being stronger and having greater momentum, last the longer.

To sum up, then, we do not think the existence of three cerebral centres for the separate perception of form, color, and white has been proven by either anatomical investigation, physiological experiment, or the manifestations of pathological change. Moreover, we fail to see the necessity for their existence, since all the phenomena of normal and disordered vision can be easily and consistently accounted for on the well-known and abundantly demonstrated laws of wave motion in the domain of molecular physics. In accordance with these laws, one cerebral centre is all that is necessary. Its molecular structure is in a condition of delicate equilibrium, which allows it to answer promptly to all the vibrations which come to it from the retina—embracing all those which answer to the colors in the visible spectrum. It is able to respond in phases peculiar to any one color, or to any combination of colors, when the resultant sensation will be of *color*. When the phase of vibration answers to all the undulations embraced in the solar spectrum, the resultant sensation will be *white*.

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[1] The so-called color-blind, who sees all colors as modifications of two, calls the sum of all his color-sensation *white*.

By a process of education we have learned to project the impressions coming from a certain part of the retina (and affecting, probably, a certain part of the visual centre) to a certain position in space. When these projections have a definite outline we have a sense of *form*.

Unless some phenomena present themselves which cannot be accounted for on this theory, we contend that we must, as scientific physiologists, accept it as a working hypothesis until there arise discoveries in the laws of the action of wave motion on molecules with which it is inconsistent. Moreover, we hold it to be dangerous to scientific advancement and truth to promulgate speculations which have not their bases in these fundamental principles.

THE AMERICAN METHOD OF GIVING POTASSIUM IODIDE IN VERY LARGE DOSES FOR THE LATER LESIONS OF SYPHILIS; MORE ESPECIALLY SYPHILIS OF THE NERVOUS SYSTEM.

By E. C. SEGUIN, M.D.

THE use of potassium iodide in very large doses under certain indications is a slowly spreading practice in this country, and constitutes, I firmly believe, a marked gain in therapeutics. By large doses I mean such as shall make up a total of from 10. (ʒ ijss.) to 40. (ʒ x.) in twenty-four hours.

This practice has been in use in New York for fully fifteen years if not longer, in a small circle of physicians whose experience has tended more and more to establish the reality of the advantages obtained by such dosage. After considerable search, and by means of personal inquiries, I have satisfied myself that this plan is originally American, and that reliable tradition indicates that the promoter and prophet of it was the late Dr. William H. Van Buren. I shall present citations from a considerable number of authorities on this matter of dosage of the iodides, in order on the one hand to establish the claim that the giving of very large doses is an American idea, and on the other hand to show how little help many of our text-books afford students and practitioners on such a vital question.

In most of the works to be cited the doses advised are absolutely too small for successful use in syphilis of the nervous system, and no specific directions are given as to the mode of administering it so as to produce the least gastro-intestinal irritation.

In the first place let us see what the leading authorities on Materia Medica and Therapeutics say on this subject.

STILLÉ: Therapeutics and Materia Medica, Phila., 1874, ii., p. 862, referring to McGregor (*Edinburgh Med. Journal*, xv., p. 309—— a mistake) says: "The success of the treatment of tertiary syphilis sometimes depends entirely upon the dose in which the iodide of potassium is administered. Cases which have only become worse or have remained stationary under the usual doses of five grains three or four times a day, will often manifest a decided progress toward cure when ten, twenty, or even thirty grains of the salt are administered at the same intervals."

CHARLES RICE: Posological Tables, N. Y., 1879, p. 62. Dose progressively increased from two to ten and to twenty grains.

SYDNEY RINGER: Handbook of Therapeutics, 10th ed., N. Y., 1883, p. 158. Five grains three times a day is generally a sufficient dose. Much larger doses, from ten, fifteen, or even twenty grains are sometimes required. For the removal of syphilitic nodes from the membranes of the brain, from five to ten grains three times a day are generally sufficient.

STILLÉ and MAISCH: The National Dispensatory, 3d ed., 1884, p. 123, states that the dose of KI varies from two grains upward; and they refer to ten or twenty grains three times a day as "very large doses." They make a bare reference to the practice of some specialists in syphilis who give more —— as high or three hundred grains per diem.

R. BARTHOLOW: Materia Medica and Therapeutics, 5th ed., 1884, p. 228, gives the dose as varying from five to sixty grains.

H. C. Wood: Therapeutics, Materia Medica, and Texicology, 5th ed., 1883, pp, 415–417. States that the ordinary dose of KI is ten grains three times a day. In certain forms of syphilis this may be increased to twenty or even sixty grains.

Trousseau et Pidoux: Traité de Thérapeutique et de Matière Médicale, Paris, 1868, ii., p. 335. Dose from fifteen to sixty grains per diem. On p. 324 they quote Ricord as giving the maximum doses.

Gubler: Leçons de Thérapeutique, Paris, 1877, p. 434. Recommends for adults from one to thirty grains (.05 to 2.) a day. " Small doses " for children.

Nothnagel und Rossbach: Handbuch der Arzneimittellehre, 3 Aufl., Berlin, 1878, pp. 271–83. The dose is .05 to 1. two or three times a day. In syphilis, consider doses of 2.50 to 3. (forty-five to seventy-five grains) a day sufficient, and larger doses, as 15. (or ʒ iv.), " as recommended here and there," as not at all necessary.

Binz: Vorlesungen über Pharmacologie, Berlin, 1884, p. 211. Iodide of potassium is best administered in aqueous solution: from .10 to 2. (one and a half to thirty grains) to be given in twenty-four hours.

Let us next see what our principal teachers of practical medicine say about this matter.

A. Flint: Practice of Medicine, Phila., 1884. The index of this work contains no reference to spinal and cerebral syphilis, or to iodide of potassium.

R. Bartholow: A Treatise on the Practice of Medicine, 5th edition, N. Y., 1884. On p. 639, speaking of the treatment of syphilis of the nervous system, Bartholow says: " In these affections the most marvellous change is wrought by sufficient doses of the iodide of potassium; no time is to be lost in its administration, and usually the largest doses are required."

A. L. LOOMIS: Text-Book of Practical Medicine, N. Y., 1884. While treating of syphilis, p. 923, Loomis says that the iodide should be increased to the limit of the patient's endurance, or until the lesion yields to treatment. Again, on p. 956, under the heading of "Pachymeningitis Syphilitica": "The iodide must always be given in large doses; from thirty to sixty grains may be given in from four to six ounces of water, three or four times daily until the desired effect is reached, which is the disappearance of the symptoms."

Before quoting syphilographers and other authorities let us go back to the physicians who originally introduced the iodide of potassium in the treatment of syphilis, and get their views.

Dr. ROBERT WILLIAMS, Senior Physician to St. Thomas' Hospital: "Lecture on the Laws and Treatment of Syphilis," *London Medical Gazette*, vol. xiv., 1833–4, pp. 39–45. Dr. Williams used KI as early as 1831 in a case of syphilis which had resisted mercury; he gave it in doses of five and ten grains in camphor mixture three times a day, with surprising results. In another case of extensive nodes upon the tibiæ and digits, with ulceration, doses of eight grains, three times a day, brought about a cure in two months. After this experience Dr. Williams' average dose in cases of periosteal node was eight grains thrice a day. Beyond that dose it purged (!). The mitigating effects of the drug were obtained in from five to ten days.

To Dr. Williams probably belongs the credit of first using and publicly recommending iodide of potassium in syphilis, but the papers of Dr. Wallace of Dublin (*vide infra*), have attracted more attention, and he is often considered the originator of the method.

Dr. JOHN CLENDINNING, Physician to St. Marylebone Infirmary, London: "Observations on the curative properties of hydriodate of potass. in periostitis and chronic articular rheumatism," *London Medical Gazette*, vol. xv., 1834–5, p. 833 and p. 866. His usual doses were from five to thirty grains, three times a day. The larger doses were given under the direction of Dr. Elliotson [1] with excellent results.

This was in a case of painful node on the cranium (case 2 of paper): as much as 100 grains a day were given with only good effects. Dr. Clendinning advises that KI should be taken well diluted, "on a full stomach and at no other time." Begins by doses of two or three grains, and rapidly increases. Remarks upon certain untoward effects: sometimes heartburn, nausea, flatulence, and diarrhœa, and rarely ptyalism.

DR. WALLACE of Dublin, "Clinical Lectures on Surgical Cases," *Lancet*, 1835–6, vol. ii., p. 5. Also in the same volume, "Lectures on Diseases of the Skin," etc., pp. 743, 688, and 894. In *Lancet*, 1836–7, vol. I., the same lectures are continued on pp. 428, 487, and 553. The observations were made in the Jervis Street Hospital. Dr. Wallace advocates the treatment of venereal disease by the hydriodate of potass or iodide of potassium ; he employs a simple solution in water so made as to give the patient about thirty grains [2.] in twenty-four hours. Has never seen any unpleasant effects. Is guided in dosage by the reaction of the urine, and does not deem it necessary to give more than will saturate the urine, *i. e.*, give a deep blue-black color-reaction when tested with dilute sulphuric acid, a small quantity of starch solution, and 1–2 drops of solution of chloride of lime.

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[1] This physician is sometimes (*vide* p. 121) referred to as having been the first to use very large doses of KI., but a reference to the original article, "Lecture on a Case of Scirrhus Uteri," in *Lancet*, 1831–2, vol. i, p. 727, shows that the medicine was not used in syphilitic lesion, but against induration of tissues. The remarks are entitled : "The Hydriodate of Potash ; quantities in which it may be safely given." Dr. E. states that it acts as a diuretic, and that he has used it with success in enlargement of the liver, spleen, bronchocele, and cancer uteri. The patient who was the object of the remarks took ʒ ij. three times a day in weak mint water, for how long a time is not stated. It is interesting, however, as corroborative of our more modern experience that these doses caused no unpleasant symptoms.

In two patients who accidentally took sixty grains a day there were severe symptoms of gastro-intestinal irritation.

In the third place let us consult some writers upon syphilis of the nervous system.

LAGNEAU: Maladies syphilitiques du système nerveux, Paris, 1860, p. 179. Considers quantities of 2. (thirty grains) a day as too small, and that this explains the want of success so often reported. He gives increasing amounts up to 8. (120 grains) per diem.

GROS et LANCEREAUX: Des affections nerveuses syphilitiques, Paris, 1861, p. 460. These authors condemn small quantities of from 1. to 2. (fifteen to thirty grains) a day as next to useless, and recommend from 3. to 10. (forty-five to 150 grains) in twenty-four hours.

ZAMBACO: Des affections nerveuses syphilitiques, Paris, 1862, p. 583. Claims to follow a happy mean between physicians who never give more than .50 (eight grains) a day and still get excellent effects, and those who give from 15. to 20. (250 to 300 grains) in twenty-four hours. His usual limit is 4. (sixty grains); in some few cases 6. (ninety grains) per diem.

FOURNIER: La Syphilis du Cerveau, Paris, 1879, pp. 609–610. Begins with 3. (45 grains) per diem and rapidly increases it to 6. and even 10. (90 to 150 grains). Says he has tried the larger doses recommended by "certain physicians," of from 12. to 20. (200 to 300 grains) per diem without any advantage. If the doses he gives are not successful the larger ones will not be (!!).

HEUBNER: Article on "Syphilis of the Brain and Nerves," in Ziemssen's Encyclopedia of Medicine. Amer. ed., N. Y., 1877, vol. xii., p. 369. Recommends "large doses" of iodide from 1. (15 grains) the first day, rapidly increased to 8. and 10. (120 and 150 grains) in the twenty-four hours.

Dowse: Syphilis of the Brain and Spinal Cord, London, 1879. In chapter v. on treatment gives no doses or mode of administration, but says he has used KI in small and in "excessive doses," but is inclined to be disappointed in his results.

E. L. Keyes: Syphilis of the Nervous System, *New York Medical Journal*, Nov., 1870. This able paper is a product of the joint experience of the late Dr. William H. Van Buren and the author, and is the first one that to my knowledge refers to and recommends the use of really large doses of iodide of potassium. The essay should be carefully studied by all interested in nervous syphilis, but for our purposes it will be sufficient to make only a few citations.

First, that in case 30, seen in February, 1870, in consultation with Dr. William H. Draper, the iodide of potassium was given in large doses, varying from ninety grains (6.) a day to ʒ ss. (2.) every two hours in an attack characterized by blindness, convulsions, and stupor, with complete recovery.

As early as in 1861, in case 31, Dr. Van Buren gave as much as sixty-seven and a half grains (4.5) three times a day with success.

Conclusion 14 of the essay states our own view of the utility and mode of using KI exactly: "That the iodide of potassium pushed rapidly to toleration, unless the symptoms subside before that point is reached, is the main outline of treatment. That mercury, used at the same time, or alternated with the iodide of potassium, is often of great value in protracted or inveterate cases; and that tonics, change of air and surroundings, frequently influence the effect of treatment in a marked degree, and may become essential to success."

Buzzard: Clinical Aspect of Syphilitic Nervous Affections, London, 1874, p. 134. "A word as to the dose of iodide of potassium. After a good deal of hesitation and trial of various quantities in a considerable number of cases, I feel convinced that in syphilitic affections of the nervous system it is often necessary to employ doses of this drug which are far beyond those usually ordered. In several instances I have observed something of the following kind to take place. An improvement up to a certain point has been produced by doses of iodide of from ten or fifteen to twenty grains three times daily. The patient has then remained at this stage, or progress has been very slow, whilst he continued to take this amount. On increasing it, however, by rapid steps, to thirty, forty, sixty, or even ninety grains three times a day, the case has responded immediately and *pari passu* to the additional quantity of the drug. I had two patients in the hospital last year who were striking instances of this effect. In each the dose was pushed gradually to ninety grains three times a day with marked beneficial effects, and I may add that the patients themselves expressed an unhesitating opinion upon this point. There is nothing new of course in the employment of large doses. Forty years ago the late Dr. Elliotson used to give as much as ʒ ij. (8.) three times a day, and with remarkably good results.[1] But of late years, as a general rule, the dose has been so moderate that to many practitioners the employment of ten grains at a time is only gradually arrived at and with some caution. No doubt in a very large number of cases a comparatively small dose is all that is required, and in practice, therefore, it is well to begin with a dose of ten grains, and increase it if necessary. I feel tolerably sure, from repeated experiments, that the iodide may be used, if occasion requires it, as freely as the bromide of potassium, and that the opportunity of doing great good in syphilitic nervous affections is nearly as often missed by the employment of inadequate doses of the former drug as used notoriously to happen in respect to epilepsy from the exhibition of too small doses of the latter.

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[1] Not in syphilis, *vide* foot-note on p. 118.

"I am in the habit of administering the drug simply dissolved in water, and have not found that any advantage is gained by the addition of ammonia or vegetable bitters." (Time of administration with respect to meals not stated.)

I have quoted thus largely from Dr. Buzzard because he is the only European writer who has had the intelligent courage to disregard tradition and give KI in doses sufficient to attain the results aimed at. He is mistaken in his reference to Elliotson, but was probably misled by some other author or by tradition in London medical circles.

Let us now seek the opinion of syphilographers.

RICORD'S doses, as cited by Trousseau and Pidoux (i., 324), were from 1. to 4. (fifteen to sixty grains) in twenty-four hours.

LANCEREAUX: Traité de la syphilis, Paris, 1873, p. 558. Recommends doses of from .50 (7½ grains) to 4. (60 grains), and even 6. (90 grains), in twenty-four hours. Denies the efficacy of quantities of 10. (150 grains) to 15. (240 grains) a day, as recommended by "some authors." The astounding reason assigned for this is that the system never utilizes more than a fixed quantity of any medicament (!).

VAN BUREN and KEYES: A Practical Treatise on the Surgical Diseases of the Genito-Urinary Organs, N. Y., 1875. These authorities (pp. 569–70) give no limit to doses; they employ (usually) a saturated solution of the iodide in increasing doses until the symptoms yield or the patient can bear no more. They cite a case in which 64. (℥ ij.) were taken daily for two weeks, with the result of checking an ulcer of the throat. They advise giving the drug after meals.

BUMSTEAD and TAYLOR: The Pathology and Treatment of Venereal Diseases, fifth ed., Phila., 1883. When speaking of the treatment of syphilitic affections of the nervous system, on p. 712, the authors urge that there is no time for half-way measures. "If the patient has not already taken the iodide of potassium, it may be well to commence with the moderate dose of 1. (fifteen grains) after each meal, for fear that he may be one of those exceptional individuals in whom the iodides exercise a poisonous influence, and if he is found to bear it well, the dose should be rapidly increased. But when his tolerance has already been tested, a dose of 2. (thirty grains), or, in urgent cases, even of 4. (sixty grains) three times a day is not too much to commence with, and it should be increased—say by the addition of .30 (five minims) every other day—until amelioration of the symptoms takes place, or at least 8. (120 grains) for each dose have been reached. At the same time free mercurial inunctions every night should not be neglected."

In their fourth ed., 1879, Drs. Bumstead and Taylor had said substantially the same thing. The iodide is to be given up to 24. (360 grains) in twenty-four hours.

From these citations it is seen that, with the exception of Bartholow (whose extensive clinical experience has given him an advantage), the leading authorities on therapeutics and materia medica do not give the necessary information as to the dosage of the iodides, and that they adhere to the *safe doses*, which are without doubt highly dangerous in a negative way, when administered to a severe case of cerebral or spinal syphilis. What is truly astonishing, however, is to find authorities like Nothnagel and Rossbach teaching *ex-cathedra*, without evidence and argument, that quantities of 15. (℥ ss.), or more, recommended "here and there," are unnecessary or useless.

The practitioner will also obtain only insufficient information from the works on the practice of medicine, with the exception of Bartholow's and Loomis.' The latter is especially emphatic : " It should be increased to the limit of the patient's endurance."

Nor with the exception of Keyes and Buzzard, are writers on syphilis of the nervous system more advanced. Indeed, one of the highest authorities, Fournier, denies the utility of massive doses which he " has tried," with a contemptuous reference to the anonymous "certains médecins" who have advocated them. Yet he is an open advocate of the heroic treatment of nervous syphilis.

Our two American authorities on venereal diseases, Van Buren and Bumstead, with their associates Keyes and Taylor reflect the advanced New York practice correctly and give, as does Prof. Loomis, doses limited only by the appearance of improvement or by intolerance.

I have remarked at the beginning of this paper that the personal recollections of some practitioners and teachers in New York, had greatly aided me in determining the origin of the practice. Let me quote the following :

Prof. William H. Draper writes me that from 1865 he has realized the utility and absolute necessity of very large doses of KI. in various forms of late syphilis, more especially ulcerating syphilides of the mucous membranes and skin, and in syphilis of the nervous system. His first knowledge of the practice was derived from the late Professor Van Buren. Prof. Robt. W. Taylor tells me that he has been cognizant of the practice of giving the large doses ever since 1866–7, when he heard Van Buren and Draper teach it at the bedside.

Prof. E. L. Keyes who was the pupil, associate, and friend of Dr. Van Buren, informs me that he distinctly remembers hearing him teach as far back as 1863, that very large doses of the iodide should be administered in nervous syphilis—

doses much larger than those then sanctioned by authorities, and nearly as large as those I am writing about.

It is true that Van Buren never wrote upon this important subject; at least Drs. Keyes and E. L. Stimson who have his literary remains know of nothing. The finger of a reliable tradition, however, points back to the distinguished surgeon named, as the originator and disseminator of a mode of dosage which has rendered much good service already.

In this matter, too, a great deal of influential unprinted teaching has been done by Professors W. H. Draper, Keyes, R. W. Taylor and others, which has disseminated the practice far and wide.

The only European writer of note who has, to my knowledge, taken the same stand, is Dr. Thomas Buzzard of London, whose excellent work is dated four years after Dr. Keyes' *brochure*, which he does not quote.

Certain writers (Fournier, Nothnagel, and Rossbach) assert that doses of 15. 20. or 30. of KI. are useless. A negative assertion is much more difficult to maintain in a matter which can only be studied experimentally, than a positive assertion. The positive facts should first be removed or accounted for in some other way, and this has not been done. Simply to state that they have tried the large doses without success is not sufficient to counterbalance the already large accumulation of successful trials.

The American method of administering KI. was developed empirically, on the firm basis of observation in practice. It was found that certain symptoms did not yield with ordinary large doses 8. or 10. (120 to 150 grains) a day; instead of abandoning the drug it was steadily increased, and it was found that the good result showed itself when the patient was taking 15. or 20., or even more *per diem*.

In another series of cases it was seen that some exceedingly acute or dangerous symptoms, syphilitic epilepsy with coma, acute cranial pain, etc., showed signs of improvement in forty-eight hours when the drug was given in doses of 4. every three or four hours.

Such facts have been often observed by the New York physicians I have named, and by myself.

No one has ever claimed that it is desirable to give large doses in all cases of later syphilitic lesions, and I would consider this an absurd proposition. As stated by all recent authorities, many cases of nervous syphilis rapidly improve and get well under moderate doses of, let us say, less than 8. (120 grains) a day. The vital question, however, is what to do for the exceptional cases; those that exhibit no change from such doses.

Let us now inquire into the indications for the use of large doses. Nowhere, to my knowledge, is this point discussed. It is simply stated, that in severe cases the dose should be rapidly increased to a maximum, — "to the limit of the patient's endurance" (Loomis). Bumstead and Taylor lay down a rule of increase (*vide* p. 123) which, I must say, would be dangerously slow in some cases. And, in certain cases too, the usual tentative small doses to determine an idiosyncratic susceptibility would entail a loss of invaluable time.

I would state the indications as follows:

1. Given a case of chronic or subacute (as regards rapidity of progress) ulcerative syphilide or of nervous syphilis, the rules laid down by Bumstead and Taylor may be followed, and the patient gradually brought to take the full doses.

2. Given a rapidly extending syphilitic ulcer, the larger doses, of from 10. to 15. per diem, should be given at once, and an increase more rapidly made in the ensuing week.

If the ulcer be in the throat much difficulty may be experienced in swallowing the dose. There are two ways out of this predicament. One is to use the rectum as has been done by Van Buren and Keyes,[1] which may be done successfully for several days if the solution be not too concentrated. Another way which I now propose would be the passage of a nasal stomach tube, and the administration of the solution through it from a funnel. The ordinary stomach tube passed through the mouth would cause much pain, and might produce laceration.

3. Given a case of extremely acute syphilitic cranial pain, whether strictly neuralgic, or from nodes, or from a deeply placed lesion. I believe, that the successful practice, and the safe practice, too, is to begin by doses of 4. (60 grains) twice the first day, three times the second, and so on. This would give on the seventh day 32. (or ℥ i.) of KI.

4. Given a case of cerebral syphilis in coma, or that peculiar stupor so suggestive of syphilis. There may also be convulsions, partial paralysis, and, in my experience, neuroretinitis. No time should be lost in such a case, and my practice is to give at once 4. (60 grains), every three or four hours, doubling the dose the next day.

5. The question of large doses in syphilitic hemiplegia is more complicated. In the premonitory period, sometimes so distinct, in which we have localized numbness or localized epileptoid spasms (Jacksonian epilepsy) there should be no hesitation; mercurials and potassium iodide should be given at once in full doses, as in cases of stupor. But when there is actual paralysis, especially when suddenly developed, the utility of very large doses is less certain. The former symptoms were of irritation, or of impending ischæmia, while paralysis often means that nerve tissue has been irreparably injured, or actually destroyed, by softening usually. Under the older teachings that most symptoms of cerebral syphilis were caused by gummata, or nodes, hope

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[1] *Op. cit.*, p. 569, foot-note.

might be entertained at almost any stage ; but since Heubner has shown that very often the lesion consists in obliteration of a large artery (endarteritis) with resulting ischæmia of a cerebral territory and its death or "softening," we must modify our prognosis and treatment. In the moment cerebral tissue undergoes the process termed "softening," it is dead, and no amount of KI. or of any other medicine can restore it. The syphilitic lesion, strictly speaking, is amenable to treatment, but its *residua* are not.

Of course, in nearly all cases, mercury is also to be used with great freedom if the case be an acute one, but the iodide is our chief weapon, and with it we should strike hard, repeated blows.

One word as to the rate of increase of the dose in chronic or mild cases where it is not likely that more than a moderate amount shall be required. Bumstead and Taylor recommend adding five grains (.30) to the dose every other day; a rate of increase which I consider as too slow even for very mild cases, or for little children. A simple calculation will show that in this manner it would require nineteen days for a patient to progress from 1. (fifteen grains) three times a day to 4. (sixty grains)—a waste of precious time in some cases. I usually increase the dose by .30 (five grains) every day; in some cases by .60 (ten grains)—that is, in mild, subacute cases.

Mode of Administration.—There is nothing to be found on this point in some text-books, and in none are sufficient details given; yet, how and when to administer a remedy are most important elements of success. As I have elsewhere[1] presented my views on this subject quite *in extenso*, I will now content myself with a few remarks.

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[1] "On the use of a feebly alkaline water as a vehicle for the administration of the iodide and bromide of potasssium," etc., ARCHIVES OF MEDICINE, vol. vi., August, 1881. "The efficacy of iodide of potassium in non-syphilitic organic disease of the central nervous system," ARCHIVES OF MEDICINE, vol. ix., June, 1883. Both papers in "Opera Minora," p. 529 and p. 579.

First, the iodide should be administered largely diluted in simple water, in a feebly alkaline water, or in milk (Keyes). This statement would, I think, be acquiesced in by all experienced practitioners, and is in harmony with the teaching and practice of many authorities. It is interesting to recall that Williams, Wallace, and Elliotson usually gave the iodide simply dissolved in mint water or in camphor mixture.

Second, the time of administration. On this point a singular unanimity prevails: give the drug "after meals," or "on a full stomach," say all the authorities. Yet, not one of them gives a reason for this direction, not even those whose ostensible object is to teach therapeutics. And, indeed, no good physiological reason could be given for this rule, which I strongly suspect is nothing more than blind following after example. In days when the physiology of digestion was practically unknown, when experimental therapeutics was not begun, the eminent physicians who were the first to use the iodide for syphilis (Williams, Clendinning, and Wallace) said give it after meals, and so it has continued. Now, I have strenuously contended against the giving of decomposable medicines, more especially the iodides and bromides, on a full stomach which contains a highly acid semi-fluid mass. It is almost a certainty, theoretically considered, that these salts are more or less split up by the hydrochloric and lactic acids of the stomach, and pure iodine or bromine set free.

The inactive stomach, on the contrary, is, we know, in just the condition to facilitate the simple absorption of saline solutions without chemical change. It is empty, and either neutral or feebly alkaline in reaction. It is capable of rapidly absorbing a large amount of simple water, and probably can do the same thing with an alkaline solution of iodide or bromide. Perhaps, also, a quantity of the solution passes into the upper part of the small intestines and is there absorbed.

Again, as to the results of experience. I find that by giving iodides in this way iodism is very rare and gastro-intestinal irritation almost unknown. Patients, who had been previously iodized by 1. or 2. per diem, given in the usual way, I found could take from 20. to 30. with impunity. Furthermore, in at least two of my patients, digestion improved while using the larger doses of iodide of potassium diluted with Vichy water, (probably because of thorough washing out of the stomach). I repeat, the iodide of potassium should always be given upon an empty stomach, say about half an hour before meals.

In the next place about the dosage for children. Influenced no doubt by the extraordinary susceptibility of little children to opiates, many practitioners give them altogether too small doses of many remedies. This is notoriously true of the bromides, and I am sure is also true of the iodide of potassium. For threatening conditions of cerebral disease, meningitis, syphilis, etc., if we decide to give KI we should administer it almost in adult doses. In cases of basal meningitis with neuro-retinitis and in some other cases, I have given from 4. (60 grains) to 8. (120 grains) three times a day to patients between four and eight years old, not only with good result as regards the cerebral symptoms, but also without iodism or gastro-intestinal irritation.

Lastly, I wish it clearly understood, that I admit that there are rare individuals in whom the iodide produces toxic effects, even in small doses. But these instances become still rarer where the remedy is given in the way I advocate. And, after all, "iodism" is in no wise dangerous, it is only an inconvenience and a drawback to treatment. Atrophy of testes and mammæ, renal disease, dental decay, are not now recognized as results of the long continued use of the drug. I have a note from my friend, Professor Bartholow, of Philadelphia, in which he tells me of an instance within his knowledge, in which a patient took 32. (℥ i.) instead of 4. (ʒ i.) at one time, by mistake. "It caused vomiting and acute iodism, but no serious after-effects."

CONCERNING THE CLIMATE OF NEW MEXICO.

By W. THORNTON PARKER, M.D. (Münich.)

ACT. ASST. SURGEON U. S. ARMY, FORT UNION, NEW MEXICO.

IT appears to one as if health resorts cease to be *health* resorts when they become easily accessible to invalids. Take for instance, the State of Minnesota, so celebrated as a sanitarium in its early days, when comparatively inaccessible, but since it is possible to reach it so easily, its fame has disappeared—the bubble has burst. The great increase of population, the destruction of forest, the damming of streams, the upheaval of the soil, these and other causes have tended to injure its reputation as a health resort.

The genuine health resort must be able to sustain its reputation for many years. In looking back at regions formerly known as health resorts, how few of them are recognized in the true sense of the term, at the present time. Physicians are constantly urging patients to places remote, wild, and unsettled. One prominent Eastern physician in one morning ordered patients to the following places: One to Colorado, one to New Mexico, one to the Adirondacks, one to the South, and another to Davos Platz; actually priding himself upon the diversity of places selected, and the tremendous distances these unfortunates would be obliged to travel. The rashness with which physicians without any personal knowledge, only from hear-say, send patients great distances is a disgrace to the medical profession, and ought to be stopped. How any man lacking practical information concerning health resorts can unreservedly recommend them and risk what remains of comfort and even life, is a stumbling block for any well-thinking man. Practical personal *knowledge* is as necessary for the climatologist as it is for the therapeutist or the surgeon. It is the well-known custom of general practitioners to refuse to attend cases suffering from severe disease of the eye. Why? Simply because they have not sufficient practical knowledge of the subject to treat the patient successfully, and do not care to hazard their own reputation or the safety of the patient by attempting it. In the case of one consulting the average practitioner for change of climate, little hesitation is shown in giving advice which may prove a total wreck to the prospects of the unfortunate sufferer. The importance of careful selection of climate in the treatment of pulmonary and other diseases cannot be over-estimated. In the *Sanitarian* for May, 1882, I have undertaken to give some practical information concerning health resorts, writing only of places I have frequently visited and carefully investigated. It is the intention of this paper to attempt some information concerning the climate of New Mexico. The sections of the Territory of which I will write are the neighborhoods of Fort Union, Las Vegas, and Santa Fé, in the northern central portions, and Forts Selden and Bayard in the south. Although these places are widely separated, the climate is very much the same throughout the Territory, the mildness of the winter of course increasing as one proceeds south.

"New Mexico is situated in latitude 31 degrees, 21 minutes and 37 degrees north; longitude, 103 degrees and 109 degrees west. It contains an area of 121,201 square miles. Large spurs, branching out from the Rocky Mountains, traverse the Territory from north to south, reaching in many instances an elevation of from 10,000 to 12,000 feet. Immense prairies, between these spurs, form the principal features of New Mexican topography." [1]

"The rest of the country [2] is a broad expanse of rolling meadow land, at an elevation varying from 7,000 to 6,000 feet, sloping off toward the south, and decreasing in elevation down to 3,000 feet above sea level. Away from the general range, mountains, valleys, and plains are more or less abruptly intermingled. In the words of Dr. Bizzell, 'Rapid transition and great diversity of elevation, containing within its border deep valleys, gorges, and cañons, associated with mountains and elevated and more or less arid plains.'

"The soil is, of course, a porous one, as is the case throughout the Rocky Mountain region.

"Water courses are few and far between. Such creeks as there are, all have their fountain-heads in the regions of eternal snow. The water is clear and supposedly chemically pure, being largely melted snow. Temperature of mountain-stream water about 58° F.

"Vegetation is as spare as it is in Northern Colorado, notwithstanding the more southern latitude. The pine growths of the mountains and mountain plateaux are not sufficiently dense to impregnate the air with terebinthine odors, and thus to be considered as a direct antiseptic agent for continuous inhalation."

ELEVATION.

"Every degree of altitude is represented, from 3,000 feet to 8,000 feet and over. The Atchison, Topeka, and Santa Fé Railroad traverses the Territory from north to south. Along its line have grown up the principal towns and settlements, representing all the above elevations. Travelling through Kansas on the same road it became clear to my mind that coming from the east through that State, and passing through New Mexico from north to south, an invalid is enabled to make a slow journey, beginning at a comparatively low altitude in Kansas, and travelling westward, to gradually ascend, until an elevation of from 6,000 to 7,000 feet is reached. This may be accomplished without deviating from a straight course westward, and yet to stop at towns of such size as to afford the necessary comforts of life, good food, society, medical attendance, and other things pertaining to civilization. To illustrate this I will give the name of such places, together with their elevations, from east to west on the railroad.

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[1] Circular issued by A., T., & St. Fé R. R.

[2] Dr. Tyndale in *Boston Med. and Surg. Journal*, vol. 108, 1883, p. 265 and p. 313.

"In Kansas; Topeka, 904 feet; Emporia, 1,161 feet; Newton, 1,433 feet; Larned, 2,015 feet; Kinsley, 2,207 feet; Dodge City, 2,499 feet; Lakin, 3,020 feet.

"In Colorado: Las Animas, 3,959 feet; La Junta, 4,117 feet; Trinidad, 6,034 feet.

"In New Mexico: Las Vegas, 6,452 feet; Santa Fé, 7,013.

"Beginning in the south, at the junction of the Territory of New Mexico with Old Mexico and the State of Texas, the figures run upwards toward the north to Raton, near the Colorado line, as follows:

"El Paso, 3,662 feet; La Mesilla, 3,844 feet; Socorro, 4,665 feet; Silver City, 5,890 feet (not reached by rail); Albuquerque, 5,006 feet; Las Vegas, 6,452 feet; Raton, 7,861 feet."

The first section of New Mexico interesting to the health-seeker is Fort Union, and it would be impossible to describe it better than Dr. Gardner, U. S. Army, who was stationed here, and experienced its trying effects on the newly-arrived. The first days and weeks are in many cases spent in suffering; the altitude is distressing, the constant wind and dust wearying, the tough, disagreeable meat and wretched food and water simply discouraging, and one is forced to ask the question in despair: What am I here for? After a while these disagreeable impressions wear off, and man, who can accommodate himself to almost any thing in this world, begins to forget his discomforts here, or else to determine to ignore them. Occupation will help to do this, and the summer weather will help to lull one's fears; but when February and March arrive the situation is indeed a serious one, and every protection and possible comfort is needed to carry the invalid or delicate person through safely until June. The experience of Dr. Gardner was much like that which I went through with after my arrival at Fort Union, and for some weeks afterward. It has been the experience of others of my acquaintance, and it must be the lot of many to suffer in the same way. The climate of Fort Union is supposed to be changing, and undoubtedly it is, as well as other sections of our country. The town of Watrouse, New Mex., eight miles south of Fort Union, would furnish very simple accommodations for summer residents, but could not be recommended at all in winter. The inhabitants are mostly Mexicans, and life would be very dull and monotonous. At Fort Union itself no accommodations could be furnished, there being neither hotel, or residents who are not connected with the military post.

"Fort Union[1] is situated in a narrow valley on the eastern slope of the Rocky Mountains, and is about 6,835 feet above the level of the sea. The soil around it is composed of fine sand, with a slight admixture of yellowish clay, and is underlaid by trap-rock and irregular beds of dark lava, which have apparently overflowed from a volcano now extinct, about thirty miles to the northward of the Post.

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[1] Dr. W. H. Gardner, U. S. Army, cited in "Circular No. 8," S. G. O., 1875, p. 303.

"Wind from some quarter is almost constant, and the soil being light and sandy, is blown about in clouds of blinding, suffocating dust, that irritates the air passages, and is the prevalent cause of catarrhs, pharyngitis, and bronchitis.

"The diurnal variation in temperature is very great, the thermometer frequently showing at 6 A. M. but 60°, and at 2 P. M. 97°; even in midsummer nights one or more blankets are always comfortable to sleep under.

"Now, from the foregoing causes, viz.: the high elevation, the constant winds, the suffocating dust storms, and the great diurnal variation in temperature, I do not believe this Post can be favorable for any kind of lung disease, and though my medical experience here is limited, I believe it will point to the same conclusion.

"The question of increased altitude as a source of disease has been one of great interest to me personally, and as I suffered as much from it myself as any other case I have seen, I will give you the history of my own case. Shortly after arriving at the Post I was attacked with a fulness of the head, ringing in the ears, mental hebitude, and confusion of ideas, dizziness, and headache. Thinking these symptoms might be caused by constipation, dyspepsia, or torpidity of the liver, I took a mercurial purgative, and followed it up with a dose of Rochelle salts, which relieved the fulness of oppression for a day or two, but it at once returned, the dizziness and confusion of ideas increased, and a feeling of numbness and tingling commenced in the fingers of the left hand and gradually spread until it involved the whole left side, even the muscles of the tongue being involved in the paralysis so that I could not articulate. There was also oppression of breathing, throbbing of the carotids, and slight dilation of the pupils. The only medicine handy at the time of the first attack was a bottle of chloroform; and thinking the symptoms might be due to spasm of the cerebral or pulmonary veins, I poured a dram or two on my handkerchief and inhaled it, when the disagreeable symptoms promptly subsided. The next day, on my visit to Dr. Moffat, of our corps, (who you will recollect was lying here disabled with a broken leg) I told him of my troubles, and he thought they were due to malarial poisoning, and advised me to commence a course of quinia and arsenic, which I at once did, taking twelve grains of quinia and $\frac{1}{10}$ of a grain of arsenic each day. But in the course of five or six days, while under the full influence of these medicines, I had another attack, in all respects similar to the first, coming on after a hearty dinner, which was relieved by a prompt emetic. Shortly after this second attack I was sent for to attend a case of midwifery at Mora (a little town in the mountains, fifteen miles northwest of the Post and about 400 feet higher in altitude), and while there alone I had another attack more severe and prolonged than the other two, and upon this occasion I certainly thought there would be another vacancy in the Medical Corps to fill, for I took emetics, bromide of potassium, and chloroform *ad nauseam* without the least effect.

"The symptoms went off before morning, but when I got back to the post I brought the Darwinian theory to bear on the case. If the environment of an animal be suddenly changed and the animal does not change its habits to suit its environment it will be speedily eliminated. The only radical change in environment I could detect here was decreased atmospheric pressure from increased altitude and consequently deficient oxygenation of the blood. The indication, therefore, was either to supply the deficiency of oxygen to the blood or to reduce the volume of blood to the decreased amount of oxygen. The latter alternative seemed the easiest and the most certain. I therefore decreased the amount of my nitrogenous food, and made up the quantity by laxative vegetables and fruits, and have been in good health ever since. I have seen two cases since, in every respect similar to mine, and they have promptly succumbed to the treatment indicated: that is, decreasing the amount of blood to the decreased amount of oxygen, by cathartics and decreased animal food.

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"One question I should like to add before closing: Are adobe quarters productive of rheumatism? I believe they are a fruitful source not only of rheumatism, but sciatica, and other forms of neuralgia."

Such is the description given by Dr. Gardner, and his experiences will be repeated very often, I have no doubt.

The following table from the Post Hospital records may prove interesting, showing the even ranges of temperature for the years 1880, 1881, 1882, 1883, and 1884.

FORT UNION, NEW MEXICO.—1880.

Mean temperature for	January	39.37	Max.	68	Min.	5
" " "	February	no observations taken (Steward absent).				
" " "	March	39.83	Max.	68	Min.	10
" " "	April	51.49	"	74	"	14
" " "	May	60.59	"	84	"	29
" " "	June	69.97	"	91	"	32
" " "	July	67.76	"	92	"	48
" " "	August	65.03	"	84	"	41
" " "	September	59.76	"	80	"	24
" " "	October	47.84	"	78	"	13
" " "	November	29.72	"	69	"	15
" " "	December	33.49	"	68	"	11

FORT UNION, NEW MEXICO.—1881.

Mean temperature for	January	26.25	Max.	62	Min.	25
" " "	February	34.68	"	67	"	21
" " "	March	37.77	"	69	"	2
" " "	April	52.86	"	75	"	5
" " "	May	60.33	"	76	"	28
" " "	June	73.77	"	94	"	45
" " "	July	71.24	"	96	"	45
" " "	August	66.80	"	84	"	40
" " "	September	59.85	"	84	"	25
" " "	October	50.47	"	79	"	18
" " "	November	33.27	"	58	"	5
" " "	December	37.16	"	70	"	0

FORT UNION, NEW MEXICO.—1882.

Mean	temperature	for	January	29.86	Max.	66	Min.	11
“	“	“	February	36.44	“	62	“	0
“	“	“	March	41.71	“	74	“	12
“	“	“	April	47.98	“	80	“	5
“	“	“	May	52.61	“	79	“	23
“	“	“	June	65.38	“	91	“	32
“	“	“	July	68.58	“	92	“	40
“	“	“	August	65.79	“	89	“	36
“	“	“	September	60.84	“	83	“	21
“	“	“	October	51.38	“	76	“	14
“	“	“	November	37.40	“	75	“	4
“	“	“	December	33.01	“	66	“	10

FORT UNION, NEW MEXICO.—1883.

Mean	temperature	for	January	29	Max.	61	Min.	30
“	“	“	February	32	“	62	“	7
“	“	“	March	43	“	71	“	10
“	“	“	April	47	“	75	“	9
“	“	“	May	58	“	85	“	21
“	“	“	June	68	“	95	“	30
“	“	“	July	69	“	90	“	44
“	“	“	August	66	“	85	“	42
“	“	“	September	60	“	86	“	27
“	“	“	October	48	“	75	“	15
“	“	“	November	41	“	71	“	5
“	“	“	December	34	“	69	“	2

FORT UNION, NEW MEXICO.—1884.

Mean	temperature	for	January	31.56	Max.	65	Min.	13
“	“	“	February	34.75	“	67	“	21
“	“	“	March	39.30	“	63	“	9
“	“	“	April	43.61	“	72	“	15
“	“	“	May		“		“	
“	“	“	June		“		“	
“	“	“	July		“		“	
“	“	“	August		“		“	
“	“	“	September		“		“	
“	“	“	October		“		“	
“	“	“	November		“		“	
“	“	“	December		“		“	

The total rain and snow fall for Fort Union for the years 1880, 1881, 1882, 1883, were as follow:—

1880, . . .	19 inches,	1882, . . .	9.41 inches.
1881, . . .	22 “	1883, . . .	14.50 “

Concerning the dryness of New Mexico the average humidity is about 38%, while in the following States and localities it is as follows :[1]

"New England States, 75%, Middle Atlantic States, 74%, South Atlantic States, 79%, Gulf States, 82%, Lower Lake region, 70%, Upper Lake region, 70%, Ohio Valley, Tennessee, and the Northwest, 73%, Lower Mississippi Valley, 58%, Denver, Colorado, 42%."

The "rainy season" commences in June and lasts through the summer and fall. By rainy season we refer to occasional showers perhaps daily for a week or so. Now and then a terrific hail storm will rage for a short time, followed by rainy and cool weather. May 31st, a severe hail storm visited this Post smashing hundreds of panes of glass and doing considerable damage otherwise. The hail stones were very large, some quite the size of eggs. The houses were flooded with water, and it was very cold for a week or two, fires being necessary in all the rooms. From the middle of June the sun shines with intense force, making out of doors occupation almost impossible, but, strange to say, cases of sunstroke almost never occur. The constant wind in New Mexico is a great protection to those who are exposed to the strong rays of the summer heat. About three or four o'clock in the afternoon clouds will appear, followed by wind or rain, and the evening and nights are cold. These sudden changes, together with the action of the water, excite a loose condition of the bowels, and diarrhœa, and even dysentery are of frequent occurrence. The high altitude affects most people very unpleasantly, at least for a time, and many suffer until they leave the country. "Nervousness," neuralgias, and all heart-trouble seem to increase. A strong desire to sleep, but awakening without much sensation of refreshment, if any. Weariness, languor, confusion of ideas, inability for exertion, or study, depression of spirits, almost to despair, are common ailments due to this climate and elevation. Fast walking and running are out of the question, and troops are not allowed to be drilled in the "double quick." Loss of appetite, indigestion, dyspepsia, and biliousnes show that the intestinal tract is affected by the change. Colds, catarrh, etc., are very prevalent; and pneumonia is rapidly and generally fatal. The houses are mostly built of "adobe," sun-burnt brick, and are cold and chilly, except in the hottest weather. Rheumatism on this account is *very* prevalent and severe. Glandular inflammations are common. Simple adenitis of the groin, without apparent cause, is very common.

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[1] Circular A., T., & St. Fé R. R., concerning New Mexico.

If I may be pardoned in this paper for mentioning servants, I would say that it is almost impossible to get any, and ladies coming from the East and attempting housekeeping without servants soon lose health and strength, if they are not permanently *aged* or completely broken down. From amongst the lazy, shiftless population of natives it is quite impossible to obtain help, and ordinary "niggers," who would not be tolerated in well-regulated eastern homes, receive from twenty-five to thirty dollars a month and are hard to find at that.

All living expenses are very high, and life at best is perplexing, discouraging, joyless, and tends to despair. There is nothing to cheer or to interest one. Even the bravest, who shot off with many plans and energetic promises, soon fall off into the dreary humdrum struggle for an existence. The desperate character of many of the inhabitants forbids a moderate investment in either cattle, mines, or farms, and the country is so covered with Spanish land grants that it is hard to obtain any just title. The whole region has, in my opinion, been over-estimated in every particular. It cannot be claimed that I have any interest in making these statements, which seem any thing but complimentary. I am not a discouraged adventurer here, neither have I any reason to speak ill of the land in an unkind spirit. I endeavor in this paper to do my duty in explaining, as fully as I am able, to the many in the East who may be ordered here and tempted to come for health and life, the state of affairs they must expect to find. Leaving comfortable homes and loved friends to come out to this wild, semi-civilized, inhospitable region, to spend all their money and hope, and at last to go back discouraged and weakened, or to be returned *dead.* Many undoubtedly come here too late, *some* come and are benefited, but, alas! how few, and at sacrifices which make life itself an expensive luxury. For the wife or mother struggling here with such patients to take care of, the blame for ordering the family out West to New Mexico, if it is to be placed on any one's shoulders, must be, or ought to be, a heavy load to carry. Some, discouraged and rapidly sinking, hasten home with wife and children, and *die on the cars.* Consider for one moment the anguish of death in such a position, and the difficulties of widow and children, cut off from the consolations and assistance of friends, perhaps without money, and at the mercy often of unprincipled scoundrels. It may happen some true-hearted man is at hand to help honorably, but, alas, it is not always, if even occasionally, the case. The death of patients amongst strangers is always hard, even at our health resorts in older lands—imagine for a moment the sickening experience in the wilds!

There is, however, one feature of New Mexican life which is to be remembered, and that is the glorious sunshine. Even in the midst of depressing, demoralizing dust storms, the sun shines out to encourage and cheer and show us that the God of nature still reigns. Then, too, the snow-capped peaks of the "Rockies," rising to a grand height, add beauty to the scene, and form a striking frame-work to the picture of this wild land. The early mornings are beautiful, and the refreshing evenings are thoroughly enjoyed.

If it were not for the blinding dust storms, which suffocate and make facing them while walking or riding well-nigh impossible, there would be much to admire; but the dust destroys all hope of falling in love with the country, and when this, the greatest evil, is added to all the others I have mentioned, life in New Mexico has verily few attractions. The poverty-stricken soil is so seldom refreshed with rain that the cultivation of vegetables is almost impossible, and those to be obtained from the Mexicans are miserable and very expensive. The meat is generally poor and tasteless. The winters draw so heavily upon the sustaining powers of the cattle, that during a greater portion of the year they are poor and tough and not fit to eat, although they are supplied as food, and no other meat is obtainable except a little mutton, as miserable as the beef. Poultry is very hard to find, and of an inferior quality.

Probably the pleasantest spot in the whole Territory, and the healthiest, unless we except Santa Fé, is the Las Vegas Hot Springs region. The Springs are beautifully situated about seven miles from the town of Las Vegas (the meadows) at the entrance to the Gallenas cañon. They are set in the midst of the foothills so cosily that violent wind and dust storms lose most if not all their tormenting power. The altitude of the Springs is six thousand seven hundred feet above the level of the sea.

The Springs are easily reached by a branch of the Atchison, Topeka & Santa Fé R. R., and the ride from Las Vegas is indeed a pleasant one. The railroad company, with considerable enterprise, have spent large sums of money in making improvements, and few places west of the Mississippi can boast so many attractions. Dr. Gordon, lately of the United States Army, is located permanently at the

Springs, and patients sent to his care will receive the best professional attention possible. There are from thirty to forty of these celebrated springs, situated at the base of a foot-hill sloping down into the Gallenas river.

"In their thermal properties they are divided into two classes: One including springs of a temperature from one hundred and twenty degrees F. to one hundred and forty degrees F.; the other springs varying from seventy-five degrees to one hundred degrees F. There are thirty of the former and ten of the latter.

"Twenty-five of the principal springs have been excavated, so as to form reservoirs, and have been walled and covered with the native red sandstone. Many of the springs are not at present required for the bath-house supply. A single spring, No. 6, furnishes, alone, thirty thousand gallons of water daily, at a temperature of one hundred and forty degrees F.

"The warm springs flow from these basins or reservoirs direct into the bath-houses, while the cooler ones run into large tanks upon the hillside, and are thence conducted into the bath-houses to furnish cold water as required.

"The skin after taking a bath in these waters has a soft velvety feeling and a freshness of aroma savoring of the cosmetic."[1]

These baths furnished at the Las Vegas Hot Springs are supposed to be beneficial especially in rheumatic, scrofulous, and other affections, and are useful in many forms of skin diseases. There are two bath-houses, one for the ordinary hot, cold, and vapor baths, well arranged and very comfortable, and the other a rudimentary affair for mud-bathing. The main bath-house is built of stone, and is really a handsome structure, two stories high and two hundred feet long by forty-two feet wide.

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[1] Circular, A., T., & Santa Fé R. R.

The bath-rooms are in the lower story, the upper being occupied by the offices of the physicians, drug-store, post-office, bath-office, etc. The bath attendants are at present excellent, and have been carefully selected. It is claimed that the entire capacity of both bath-houses is 1,000 baths per day, but of course the number of bathers do not require so much attendance as yet.[1]

Separate apartments with complete bathing apparatus are furnished those suffering with contagious diseases.

The baths are given at a temperature of from 90° to 100°.

The mud baths given at these springs are similar to the celebrated peat baths of Franzensbad, Bohemia. "The percolation of chemical water for centuries through the earth surrounding these springs has produced this peat, or mud. Viewed under the microscope it appears gelatinous, its earthly properties having undergone a change."

The average temperature of the mud bath is about 108°.

One hundred and six degrees is probably the best, and is very agreeable.

The average duration of the bath is one hour, but the time may vary from thirty minutes to three hours. One bath is usually taken each day. Upon leaving the tub the patient is washed off thoroughly under a graduated douche, and after being "hosed off," is then rubbed down clean and dry, and placed on a comfortable couch in the "resting room" for half an hour. The bath is supposed to act like

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[1] The baths, including attendance, towels, etc., cost as follows :

Shampoo baths	*Each*	$1 00
Electric "	"	1 00
Medicated "	"	1 00
Vapor "	"	75
Vapor and pack baths	"	1 00
Tub baths	"	50
" and pack baths	"	75
Mud baths, singly	"	3 00
" " three for $5 00 ; five for $8 00 ; seven for $10 00, and ten for		12 50
Special treatment, Massage		7 00
Massage at time of taking bath		50

Bathing hours, 6 A. M. to 6 P. M.

On Saturdays, bathing hours 6 A. M. to 10 P. M.

a huge poultice, extracting by free diaphoretics, or sweating, diseases that have baffled skilful physicians both at home and abroad. The pack and massage are often ordered to follow the mud baths as a re-inforcer.

In preparing the mud baths, the mud is first freed from all foreign substances, such as sticks, stones, etc. Each tub is then half filled with mud, and mixed up with the warm spring water, to the consistency of paste. The bath is warmed up by steam at bathing time to the required degree of heat ordered by the physician in charge of the patient. Every five days *fresh* mud is ordered, and the old mud cleared out. It is amusing in passing through the bath-house to read the labels. Mr. S.'s mud: Rev. Mr. D.'s mud; Mrs. A.'s mud, and so on. The boxes look like coffins, and the bather is held down in the tub by a board placed across the chest and firmly fastened down; otherwise he will rise on top of the mud. It is said that at first ladies shrink from entering the repulsive stuff, and can hardly be forced to do so; but having once enjoyed the bath, it is hard to keep them away from it!

At the Hot Springs there are several local attractions in connection with the hotels—museum of antiquities, aviary, zoölogical collection, etc., besides a large green-house, well filled with floral and variegated plants. The hotels at the Springs are in postal, telephonic, and telegraphic connection with Las Vegas and the outside world. The hotel rates are from $3 to $4 per day—$17 50 to $24 a week. There are several cottages upon the grounds where private board at reasonable prices can be obtained.

The next most desirable residence for the temporary visitor to New Mexico if not altogether the best place in many respects, is the old city of Santa Fé. It is at first a forbidding place but one learns to like it, and although the native inhabitants are mostly dirty uninteresting people, really delightful society is to be found in Santa Fé the year round. Living expenses are high in Santa Fé as elsewhere in the West, and servants few and hard to find—but compared with all the other towns in New Mexico, Santa Fé is certainly the most comfortable. The same climatic advantages and drawbacks exist here as at Las Vegas and Fort Union and other New Mexican towns. I attach to this paper another report from the "Army Records" showing the characteristics of the climate and some of its disadvantages:

"The city of Santa Fé[1] is pleasantly situated on an extensive plateau on the western slope of the Rocky Mountains at an elevation of 6,850 feet above the level of the sea, and in latitude 35° 41′ north, longitude 28° 59′ west. To the north and east rise the foot-hills and peaks of the Rocky Mountain range; on the south the plateau is gradually lost in spurs of mountains shooting out of the main range, and on the west it terminates somewhat abruptly in the valley of the Rio Grande. * * * Fort Union is distant about 100 miles to the eastward. * * * To the south runs the main road to Albuquerque and Southern New Mexico, and less important roads to the neighboring towns. The Rio Grande runs in a southwesterly direction from Santa Fé about eighteen or twenty miles distant at the nearest point. A small mountain stream—a tributary of the Rio Grande—called the Rio de Santa Fé nearly bisects the town. The soil of Santa Fé and vicinity is dry, light, and sandy and yet very fruitful. The country for miles about Santa Fé is destitute of trees. The large growth is said to have been cut away at an early date in the history of the place for fuel and for better security against hostile Indians, and a subsequent growth of large trees has not appeared, though stunted cedars and pines are very common. This want of vegetation detracts much from the natural beauties of the town and vicinity. On the hills toward the mountains are found large pines and cedars. The piñon, a species of pine, furnishes the almost sole supply of fire-wood. It is brought for miles on the backs of donkeys, and sold by the load in the plaza at from twenty-five cents to one dollar, according to the season of the year or severity of the weather. The natural drainage of Santa Fé is excellent, and is materially assisted by an extensive system of *acequias* or canals around the town. Still, little attention is paid to the subject and many of the narrow streets and lanes of the city are excessively filthy. The river water is very extensively used for drinking purposes and is excellent. Good water, but a little impregnated with lime, may be obtained by wells at a depth of from ten to forty feet.

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[1] Information concerning Santa Fé, New Mexico, furnished in "Circular No. 8," Surgeon-General's Office, by Surgeons Alexander and Huntington, United States Army.

"The population of Santa Fé is about 6,000, of which the larger portion is Mexican and Indian, or an admixture of the two. The American element is rapidly increasing and already has the chief influence in matters of trade and politics.

"The place is irregularly built of adobe, and when seen from the approaches of the town has an exceedingly uninviting appearance. The houses are generally built on the Spanish plan, a quadrangle with an interior court yard, the entrance being through a gateway generally kept closed. The older portions of the town are built upon narrow lanes and passage-ways rather than upon streets. The better portion is the more recent, and is inhabited by the American residents. The plaza holds a conspicuous place as a business centre and about it are the civil and military offices."

To the excellent description from which I have just quoted is appended a communication from Dr. Smith, of the United States Army concerning the climate of Santa Fé for invalids. His remarks are very valuable and should be given considerable weight in selecting this climate for pulmonary patients.

"From an experience of fourteen months and upon rational grounds, I cannot coincide in the popular belief that Santa Fé and the contiguous localities, of equal or superior altitude, are well adapted as a residence for persons suffering from pulmonary tuberculosis, heart disease, or any cause producing obstruction to free and ample respiration. The universal testimony is, so far as I can ascertain, that a stranger to the rarefied atmosphere, however sound his pulmonary and circulatory organs may be, is almost invariably affected by a great oppression in respiration upon his advent into this elevated country, accompanied naturally by an unwonted lassitude and indisposition for exertion.

"There have been in the case of two or three of my acquaintances ugly symptoms of a partial paralysis of the organs of locomotion and speech. A continued residence, however, is said to overcome these unpleasant effects in persons of *sound* and *robust* health, and from the number of Americans and Germans residing in the higher regions of New Mexico, who transact their business at no small expenditure of physical exertion, I believe this to be the case, and that *in time* an accommodation obtains between the lungs and the somewhat diminished quantity of oxygen.

"As regards the *invalid*, whose breathing apparatus is crippled by tubercular deposit, by chronic pneumonia, or whose blood, whatever may be the cause, requires full aeration, I deem it worse than useless for him to endeavor to regain health or even comfort in such localities. I regard my lungs (and my chest measurement is forty-four inches) as perfectly sound, and yet, after reporting for duty in Santa Fé, I could not, as a general rule, breathe comfortably, although at times, when a damp atmosphere prevailed, I could not notice any impediment to respiration. The past summer (1874) was exceptionally warm and I was at intervals asthmatic to a terrible degree, crushed actually by a feeling of impending dissolution. The common advice to me was 'Wear it out; you will be all right next year.' No sooner, however, had I started East than my troubles, as I descended in altitude, lessened proportionably."

The places south of Santa Fé in my opinion become less and less desirable, although one seeking a very mild winter climate might try Silver City if suitable accommodations could be found. For a single man a certain kind of living might be put up with for a time, but for the invalid, or for an Eastern family used to comforts and even luxuries of life, a residence further south than Santa Fé or Albuquerque is, in my opinion, undesirable. The Rio Grande valley has some advantages in its mild winter and absence of snow. The summers are said to be cool and pleasant, but when I rode through on horseback in 1867 I found the climate hot enough. The best opinion concerning the climate is to be found in the reports of the medical officers to the Surgeon-General's office and those on Fort Selden and Fort Bayard now presented. The climate was the same then as now and the officers who made the reports did so only after careful investigation.

"Circular No. 8, Surgeon-General's Office, Fort Selden, New Mexico." Report of Dr. Jessop, U. S. Army :

"Fort Selden is situated on a sandy basin one and one half miles from the Rio Grande, in southern New Mexico. Latitude 32°25′ north ; longitude 30° west ; altitude 4,250 feet. Fort Cummings, fifty-five miles west—Fort Bliss, Texas, sixty-seven miles south (nearest town La Mesilla).

"Lung troubles are comparatively rare at Fort Selden, as are all diseases of the respiratory organs, excepting catarrh, which I prefer to consider separately from bronchitis, as it mostly affects the mucous membranes of the nares, tonsils, and larynx, and, I think, seldom extends even to the trachea. It seems to be produced by the almost constant drifting of the irritating dust peculiar to this region, and few new-comers who are much exposed in the open air escape it. It gradually wears off as they become acclimated. Women, from their in-doors habits, are usually freer from it. The native New Mexicans are not at all affected by it. Many of them, however, suffer from a form of bronchitis, induced it is thought, by their peculiar fashion of smoking the cigarita, *i. e.*, by inhaling the smoke into their bronchial tubes and exhaling through their nostrils. The climate of this part of the valley of the Rio Grande will improve and probably tend to the cure of many patients afflicted with commencing phthisis, but only by a residence here, not a sojourn of a few months. I think I am within the mark in stating that it will take from eighteen months to two years to acclimate them. To those in advanced stages of consumption, no such benefit can accrue. Added to the annoyance of the dust-storms, are the distance from home and the impossibility of obtaining many of the comforts, and especially the varieties in food, which the sick always crave. The prognosis is extremely serious if diarrhœa be a complication, and here I may observe that all cases of chronic diarrhœa appear to do badly at this Post, and that, with my present convictions, I would not suffer, if it could be avoided, a patient laboring under this disease to remain at Fort Selden or any point where the Rio Grande constituted the water supply. The enervating nature of the climate has doubtless much to do with the ill-success attending the treatment of such patients. As regards chronic bronchitis, I can only speak for New Mexico, and, for that disease, I consider the climate of this region as the best in the Territory."

Near Ft. Bayard is a bright little town called Silver City, which I have already briefly referred to. Grant county is considered one of the best counties in the Territory, and the business enterprise of Silver City is very much in its favor. The town is pretty well supplied with stores, and living is not quite so tiresome here as at many other places in the Territory. Ft. Bayard is situated in the extreme southwest corner of the Territory, in latitude 30° 40′ north, longitude 31° 25′ west, at an altitude of 6,022 feet.

The military Post is one of the most delightful in the

West, and a great favorite with the Army. Diseases of the heart do badly here, and the climate, excepting that it is milder in winter, is much like the northern sections already described. It is a long way from home for the invalid, and summer "*wash-outs*" on the railroad make the States seem very far away indeed sometimes.

"Circular No 8, War Dept., Surgeon-General's Office, 1875. Fort Bayard, New Mexico." Report of Dr. Wilson, U. S. A.

"In bronchitis, either acute or chronic, this climate is unfavorable either for cure or relief. The air is too rarefied and too stimulating, and acts almost as an irritant to the bronchial mucous membrane. I have also observed in even slight cases of catarrh attended with cough, that they are very intractable. I have seen several cases of phthisis, and have one at the present time under my charge, but I have not seen any beneficial results produced by this climate. They have all gone on from bad to worse, and finally died. I believe that it is only in the very early stages of tubercular disease that this or any other climate can exert a beneficial influence. * * * There are, so to speak, two classes of cases, one of which is characterized or accompanied by a dry hacking cough, but with little expectoration and a tendency to hæmoptysis. In the other, there is copious secretion from the bronchial mucous membrane. This latter class would be benefited by a mild, dry, climate not subject to sudden changes and of a lower altitude than this, say 2,000, or 3,000 or 4,000 feet. The former class would lead a life of torture here, and this climate or one similar to it would only add to the mischief already done. They would be benefited by a mild, moist climate in close proximity to the sea. Phthisis is almost unknown among the Mexican population here, notwithstanding their filthy habits, probably on account of their living nearly all the time out of doors and being natives of the soil and accustomed to this climate. I know that horses brought here from the States, and cattle introduced here go down in condition for the first year until they are acclimated, and I believe that the human race require also a certain time for acclimation."

In summing up the *advantages* of the New Mexican climate we find first a considerable degree of *elevation*. Dryness is very decidedly present. Vivenot[1] classifies moistures as follows:

Dry, below and up to 55%.
Moderately dry, 56 to 70%.
" moist, or moist, 71 to 85%.
Excessively moist, 86 to 100%.

Referring to the tables taken from A., T., and St. Fé R. R. circular we see that New Mexico can boast of a *very dry climate*. The percentage of clear sunshiny days is very great, and the amount of inclement, disagreeable weather is proportionately small. One can get out of doors for a considerable part of the winter. The *disadvantages*, and they are many, have been mentioned. The principle one is the *great elevation*. Here we have the advantages and the disadvantages of *elevation*, and it is hard to say which are greatest. It is worth while to mention that when "the country in the northwest, middle, and eastern States, is ice-bound and frozen, the residents of New Mexico are basking in genial sunshine."

Undoubtedly this paper may be read by English people contemplating a visit to the Southwest. To those used to the climate of England, only excepting the beautiful Isle of Wight and the grand south coast, New Mexico will prove an interesting, and I believe, in many respects, a profitable change; but—and it is a large *but*,—we have in the United States many desirable climates: the Adirondacks, the Mountains of Tennessee, the regions of Moosehead Lake, Montana, and last, but not least, the glorious climate of Colorado. The warm climates may be good, and the mild winters desirable, but the evidence lately seems to show that the cold clear climate of Colorado is the best for cure, and the best in the end, and the happiest home for the invalid obtainable. In coming to this climate and to this country, and leaving behind the comforts and luxuries of the last, and accepting Western hospitality is a great change indeed.

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[1] Quoted by Dr. Tyndale in *Boston Med. and Surg. Journal, l. c.*

To the average Englishman, accustomed to his comfortable home and well-regulated table, be he in robust health or an invalid, the American hotel, and especially in western towns, is simply discouraging. To the West people flock for money-making, and personal comforts are of secondary consideration. The invalid must take his chances with the rest. Rents are high and separate houses are hard to find—one must choose between the hotel and the boarding-house. The first-class hotels are, of course, very fair, but the charges are exorbitant, considering what one gets. The boarding-houses are simply unendurable, both as regards food and accommodations generally. Society is, of course, very much mixed, although there are always some agreeable people at all places.

For one, however, willing to endure hardship and take life just as he finds it, and with plenty of means at his disposal, and who can plan and carry out the journey comfortably, it may be well to seek the American health resorts of Colorado or New Mexico. When we consider the bright sunshine, so generally present, and the invigorating, healing atmosphere, which is especially obtainable in Colorado, and the opportunities for out-of-door exercise, and the possibility of regaining health, the journey is worth all the trouble and expense and suffering. It is important to know where to start for, and the proper route to take, besides the time of year and the expense. It is best in going to Colorado to make Denver the first objective, and for New Mexico, Santa Fé or Las Vegas Hot Springs.

The hotels are constantly improving, and better attention is paid to the comfort of guests. New houses are constantly being built, and conveniences, which are so necessary for the invalid, are also increasing each year, and better protection against the winds and dust are constantly being afforded. A well-filled purse is, however, of the first importance, and to creep along with insufficient means only brings unhappiness and disaster upon those attempting it. Certainly it is quite unnecessary to banish any one to the Davos Platz, which does not begin to afford the advantages to be found in Colorado or New Mexico, and is nearly if not quite as expensive for the Englishman or American as a trip to the far West.

For the invalid going either to Colorado or New Mexico, one word of caution is necessary, and that is: lessen the dangers of hemorrhages by approaching the elevated regions *gradually*. Many lives are undoubtedly sacrificed by this hasty *rush* from New York to Denver or Santa Fé. The sleeping cars are luxurious and the dining cars excellent, but a rest for a few days in Chicago or Kansas City, and another rest half-way before reaching Denver or Las Vegas is most desirable, and will pay in the end without doubt.

The best season to go West is in the spring or fall, to be somewhat acclimated and settled before winter, and to avoid for the invalid the journey in hot, dusty cars in summer is not to be lost sight of. A well-filled lunch-basket, and a flask of good wine and another of brandy, besides some preparations of easily-taken beef-essence, like the London Manufac. Co.'s preparation, or Valentine's meat-juice, is very necessary for the safety of the invalid, and should not be overlooked. A preparation of coca wine, made by Theodore Metcalf & Co., of Boston, is the best single preparation for travellers that I know of.

This preparation of wine of coca of Metcalf's is desirable for the invalid after reaching either Colorado or New Mexico. Coffee and tea are too stimulating, and exert an injurious influence on the nervous system in these high altitudes. Coca is desirable as a sedative to the nervous system, and at the same time a delicious, invigorating tonic. Constipation is very apt to trouble the traveller not only on the journey, but after his arrival in the new country, and some easily taken cathartic, like Brewer's tartrate of soda, in effervescing granules, or the new and excellent cathartic also manufactured by Metcalf & Co., of Boston—Rhamnus Frangula—will be found very valuable, and will obviate those distressing headaches and general malaise which a long journey is apt to induce. The traveller needs few medicines besides these mentioned. He is seeking for the climate cure, and if he obtains that to his satisfaction, the battle is won.

The hunting and fishing in New Mexico is not to be boasted about. Game is not plenty, and hunting can only be found away from the settlements and at considerable expense and trouble. There is some, to be sure, and the devoted huntsman will find it.

There are many interesting excursions to be made, and the ancient Pueblos offer considerable that is interesting in their history and customs.

As to business enterprises, I should hesitate a long time before advising any one to assume the risk either in "cattle and sheep ranches, mines, or manufactories." For professional men and especially doctors the prospects are gloomy in the extreme, and visitors are not at present numerous enough to offer any inducement to hope for practice from them; the native population would, to say the least, be very undesirable patrons. The schools are of course very inferior, and the territory of New Mexico is not a desirable place to bring up a child in the way he should go. To make the experiment of New Mexico for health is a lottery indeed, and while many may lose their all, *life and health may be found!*

The Atchison, Topeka, and Santa Fé Railroad from Kansas City is *the* route to New Mexico. It is a well-managed, comfortable, and even luxurious road, and the welfare of the travelling public is constantly attended to. The eating-rooms at the stations all along the road are excellent, and directly under the railroad management.[1]

I have tried to explain the situation to the best of my ability. The physician must choose for his patient, and the patient must get the best information obtainable. To those going to New Mexico either for health or pleasure, there is much to be seen and learned of which this short paper cannot treat. To all a pleasant, profitable journey is most sincerely wished.

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[1] The following is a schedule of rates to Las Vegas Hot Springs :

	1st class.	2d class.	Round trip, good for ninety days.
Chicago	$46 15	$35 65	$57 80
Kansas City	31 35	27 75	37 80
Atchison, Kan. . . .	31 35	27 75	37 80
El Paso, Tex., to Hot Springs			22 80

ILLUSTRATIONS OF THE ANOMALOUS COURSE OF POSTERIOR SPINAL SCLEROSIS.

By E. C. SEGUIN, M.D.

THE typical or normal course of the disease variously known as posterior spinal sclerosis, progressive locomotor ataxia, or more briefly, tabes dorsalis, has become generally known to the profession by the dissemination of recent text-books of diseases of the nervous system and of monographs upon the disease in question. The writings of Charcot and Erb particularly have been of service in thus rendering familiar the numerous symptoms of tabes and their grouping into three stages, viz.: the pre-ataxic or neuralgic stage, second the ataxic stage, and third the pseudo-paralytic or bed-ridden stage.

I prefer grouping the symptoms of typical cases into two stages only, viz.: a neuralgic pre-ataxic stage, and a second or ataxic stage. The so-called third stage is simply an aggravation of the second without any new distinctive symptom.

I do not design to recapitulate the symptoms of posterior spinal sclerosis in this paper, chiefly for the above reason, and also because such a summary is accessible to every reader in Hammond's treatise on diseases of the nervous system, and in a lecture of my own on the "Diagnosis of progressive locomotor ataxia."[1]

Non-typical cases of posterior spinal sclerosis while by no means rare, are yet often puzzling and misleading. A record of them is desirable, and remarks on their semeiology and probable pathology may not prove wholly devoid of interest.

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[1] "Series of American Clinical Lectures," vol. iii., No. 12., N. Y., 1878. Seguin, "Opera Minora," p. 353, N. Y., 1884.

Numbness, paresis, and ataxia preceding fulgurating pains: antecedent scoliosis and syphilis.

Case I.—A male æt. 35, a broker by occupation, single, seen in September, 1878. Former health good; from youth has had an extreme right dorsal lateral curvature of the spine caused by persistent over-use of the right arm. Fourteen years ago, when 21 years old, had a chancre followed by some cutaneous eruption, but not by sore throat or osteocopic pains. Remained well until two years ago, when slight numbness appeared in the left foot and leg, and progressively increased. The right foot became involved only in the last few months. During the past summer has noticed progressive weakness of legs, slow micturition, diminution of virile power. Has noticed no abnormal reflexes, spinal pain, cincture feeling, symptoms in hands or head.

Admits no sexual excesses and no injury to spine. The curvature is as it has been for many years.

No neuralgic or fulgurating pains in legs.

The examination showed weak but not ataxic legs; left leg weaker. Marked anæsthesia of soles of feet and toes to æsthesiometer and needle. No muscular atrophy. The patellar tendon reflex and that from the soles of the feet are both lost. No mention of state of pupils.

A thorough anti-syphilitic treatment by means of mercury and iodide of potassium was carried out very carefully by the patient's physician, Dr. T. E. Satterthwaite. This was repeated afterward from time to time.

The consecutive notes of this interesting case are incomplete, but as the patient is still under observation I can give a fairly correct idea of the course of his disease. The paresis passed away in great measure. The numbness and partial anæsthesia remained, extending to the knees. Distinct ataxia, aggravated by closure of eyes, developed in the second year of observation (third of disease), and the first fulgurating pains not till 1880 or 1881, and these were rectal pains, apparently neuralgia of the rectum. Since 1881, occasional pains, sharp, momentary, or burning, in patches, developed coincidently with a low barometer, have occurred in the thighs and legs. These have never been severe or long continued; in marked contrast to the usual course of tabes.

The bladder and sexual organs recovered their power and remain normal. This, together with increased firmness in gait, constituting a veritable arrest of the disease, was brought about by nitrate of silver internally, spinal galvanization, and more especially by a course of spinal douches, done under the direction of Prof. Charcot in Paris during 1881.

At no time have the eyes presented symptoms; no diplopia, amblyopia, or Argyll-Robertson pupils. The arms remain normal. During the past winter only a few neuralgic pains have occurred, there was some increase in numbness and staggering, but the ataxia was no greater, and the patient has been able to attend to his business as a broker (office work only) with great regularity.

At the beginning this case presented the clinical picture of a subacute syphilitic myelitis in the lower part of the lumbar enlargement. In its second stage, where it now stands arrested (?) the symptoms consisted in ataxia, partial anæsthesia, absence of patellar reflex, and slight but characteristic fulgurating pains in the rectum and legs. Entirely abnormal to the usual semeiology of tabes are the absence of symptoms about the eyes, and the inverted order of appearance of the pains.

It might be argued that the case was one of central myelitis very low down at first, with secondary changes in the posterior columns, or more properly the columns of Burdach for a certain distance up the lumbo-dorsal cord. Considered in this way, the case (as well as the following) might be taken as favoring the theory that posterior spinal sclerosis is sometimes a secondary and degenerative lesion.

As regards therapy and prognosis the case is interesting. Anti-syphilitic treatment was most clearly called for, and it was carried out very thoroughly, the gums being slightly touched by mercury and iodide of potassium given in quite large doses up to 15. per diem. And the same remedies were used more than once in smaller doses afterward. The symptoms of myelitis apparently yielded in part to this treatment, but the more strictly tabetic symptoms appeared and persisted. The unmistakable improvement, or check of the disease was obtained later by remedies which are more especially of use in spinal sclerosis, viz.: spinal douches, galvanism and nitrate of silver.

Acute double sciatica, followed by ataxia of the legs.

CASE 2.—A male, aged thirty-two years, married, a merchant by occupation. Seen July 6, 1880.

Was perfectly well until three weeks ago. Had a chancre in 18—. Developed ten days after coition, followed only by "a few pimples."

About June 16th, after a hard day's work in New York calling on many friends, became much heated, and drank freely of cold lemonade. The next day had pain in abdomen and took Congress water without effect. On Tuesday, June 22d, had very severe pain in both sciatic regions, extending to the toes, most acute under the knees ; no numbness. On the 24th came to the city from his country home, with same pain in both sciatic distribution ; a sense of painful pressure and burning in the epigastrium. No loss of power in the legs. Iodide of potassium and salicylic acid were given freely without relieving pains, which were so severe that he had to use laudanum to obtain any rest. The pain was continuous, not stabbing or fulgurating. On the 27th still suffering the same bilateral sciatic pains ; numbness appeared in the feet, rising steadily. On 28th could still walk pretty well, but from the next day (29th) noticed "weakness" of legs ; the bladder acted slowly and imperfectly ; there was pain in the dorsal part of the spine extending into the epigastrium. In the last two or three days the spinal pain has extended to level of the shoulders. The sciatic pain has ceased. The numbness is severe up to knees, and there is slight loss of sensibility of the skin, to level of waist ; a trace of numbness has appeared in the fingers, cannot stand or walk without assistance. Has partial retention of urine and constipation. The muscles of the lower limbs are uniformly though but slightly atrophied. In sleep frequent jerking of legs.

Examination showed some paresis and slight uniform emaciation of the lower extremities. The patient can stand with assistance, but closing his eyes makes this impossible. The bladder is full of urine, and the catheter draws off three pints. There is neither plantar nor patellar reflex. The chief symptom, however, is typical ataxia of the legs in the attempts to stand and in tests performed while lying on the back.

Without complete anæsthesia, there is marked diminution of sensibility, especially in the right lower extremity. The feet feel numb. No pupillary or other ocular symptoms, and no symptoms in the upper extremities.

The patient had had a chancre some years before, not (?) followed by secondary symptoms; his body is free from cicatrices, and he has two children who are pictures of health. (A third healthy child was presented him by his wife in the year after his attack.)

The patient denied in the most formal manner having had any pains in his legs prior to the attack.

Treatment consisted in the systematic emptying of the bladder twice a day with carbolized catheters; the use of small doses of mercury, and of full doses (up to 5. three times a day) of iodide of potassium, and the application of the galvanic current to the spine and legs. At the end of July was given strychnia.

A peculiar symptom was a severe "griping" pain in the left side of the thorax, from the shoulder forward at the level of the seventh and eighth intercostal spaces. The time of appearance of this pain is not noted, but on July 8th it is "nearly gone," and afterward gave patient very little trouble.

On August 8th the first attack of fulgurating pain is noted, as a stabbing, cutting pain in the internal aspect of the left knee, lasting eight hours.

The case was under observation and treatment two years, during which a certain improvement occurred. The muscles of the legs regained their size and power, the sensibility in a great measure returned, and the bladder (after two attacks of cystitis) recovered its function. But the ataxia remained, though diminished, as the patient was when last seen able to walk with one cane on a sidewalk or piazza several hundred yards, though with characteristic jerk. The fulgurating pains were experienced occasionally during the two years, less frequently, but just as severe and typical. The left side of thorax, though free from pain, was still the seat of a pressure paræsthesia.

This interesting case was quite tabetic in its character after the first eight weeks of its course, the symptoms then being fulgurating pains, ataxia, slight anæsthesia of the legs and absence of patellar reflex. In its beginning, however, it is quite anomalous, and it is quite probable that the primary lesion was one localized upon the posterior columns of the cord at the end of the seventh or eighth dorsal vertebræ, mostly on the left side of the median line. Such a lesion would account for the severe epigastric and abdominal symptoms experienced at the beginning (dorso-spinal and gastric sympathy is well illustrated in spinal irritation cases), the distinctly localized pain with sense of griping or grasping in the left side of the thorax, in the seventh or eighth intercostal spaces. Such a lesion would also pretty well account for the seeming double sciatica. Many years ago, Cruveilhier showed that *paraplegia dolorosa* was almost pathognomonic of tumor compressing the spinal cord.

This supposed lesion was checked by treatment (?) but not before the conducting power, both for sensibility and for coördination, of the posterior columns had been seriously impaired. In the third week, when first seen, the ataxia of the legs was already extreme. There were also other signs of compression of the dorsal spinal cord, viz.: paresis of the legs and of the bladder (retention). In the seventh or eighth week, and later, the fulgurating pains showed themselves in various parts of the lower limbs, indicating slow sclerotic (or degenerative?) changes in the columns of Burdach below the level of the eighth dorsal nerve.

It is interesting to note in support of the theory of a primary localized lesion that while the thoracic pain was upon the left side, the opposite, right lower extremity was more anæsthetic than the left.

In spite of the absence of secondary symptoms and of transmission of syphilis to offspring and wife, it is possible that the lesion was a specific formation either in the dura mater or more probably in the external layer of neuroglia. Its increase was arrested at a certain point, but not before irreparable damage had been done to delicate nerve structures. The question has always been in my mind, would a very active treatment, by means of mercury and large doses of iodide of potassium at the very beginning of the spinal symptoms (fifth day of attack), have saved the patient's legs from permanent disability?

Precocious atrophy of optic nerve; locomotor ataxia.

CASE 3.—Mr. G. C., æt. thirty-six, seen in August, 1878; referred by Dr. Thos. R. Pooley.

About four years ago simultaneous appearance of diplopia, tendency to stagger or walk badly, and specks before eyes. Is positive that at that time and previously he had had no pains in his legs. In the course of a year the diplopia, which was due to paralysis of the left sixth nerve, diminished, but the sight of the left eye failed. From that time vision gradually became worse until lately he has only had perception of light. Dr. Pooley finds extreme atrophy of both optic nerves.

In the last few months patient has had somewhat sharp pains in patches upon the legs and thighs, occurring semi-periodically. The urine has been passed slowly of late. Denies any feeling of numbness in feet, but has had a sensation as of a ball under his feet, and has felt as if treading on rubber. Patient is aware that his staggering is not accounted for by blindness.

Sixteen years ago he had a chancre followed by "warts at the anus," but no other symptoms. Children healthy.

Examination: Optic nerves atrophied, but pupils are "equal and normal" (notes taken at the time). No symptoms in arms. Legs strong but distinctly ataxic; more so when eyes are closed. The soles of the feet are slightly anæsthetic, but pricking is well felt. Absence of patellar reflex. No arthropathies or muscular atrophy.

This case belongs to a category which I suspect is not small. At least I have met with several such in which the amaurosis was coincident with or antecedent to the ataxic stage. At the present time there are two cases under my care in which, with very little ataxia, there exists complete loss of vision, in one case with atrophy of the optic nerves, in the other with slight ophthalmoscopic signs.

What is interesting to note is that in such cases we have not to do with an extended longitudinal sclerosis (at least judging by clinical signs), for in these three cases the arms remained normal at the time of last examination. The lesions in the optic apparatus and those in the dorso-lumbar cord doubtless arise under the pathogenetic law of sclerosis, which at present is wholly unknown to us. Cases of this variety, in which the optic apparatus suffers only, and the more numerous cases in which paralysis of one or more ocular muscles precedes (sometimes by years) the first or neuralgic stage of tabes, constitute an almost insuperable objection to the theory that posterior sclerosis is a degenerative lesion secondary to disease of peripheral nerves—an ascending degeneration.

Extraordinary prolongation of the pre-ataxic or neuralgic stage (29 *years*).

Case 4.—In 1878 I was consulted by an artist, 57 years of age, for a "neuralgia," which had tormented him from his thirtieth year. The pains affected the lower extremities only until two or three years ago, when they showed themselves in the arms as well. Patient describes these pains as sudden, sharp, tearing, sometimes of atrocious severity, occurring in spots or patches of round or oblong outline. These pains recur in one spot for some time, varying from a few minutes to hours and days. In the course of these many years he has had foci of pain in nearly every part of the lower extremities, more especially near the knees and ankles. In the last few years the intervals between paroxysms have become shorter, and the pains have grown more severe. There is now mydriasis of the right eye, a condition which has existed thirty years without diplopia. A mere trace of numbness has made its appearance in the legs, detected only at times by rubbing the skin. The painful spots are hyperæsthetic during the paroxysms. In the last few years the urine has passed slowly. The floor or ground feels normal under foot ; no difficulty in walking. Examination shows dilatation of the right pupil, without diplopia ; no changes in the optic nerve (patient worked at his art to the last). No ataxia of the upper and lower extremities. There is very slight staggering when patient attempts to stand with eyes closed. The soles of the feet show slight anæsthesia to æsthesiometer test. Reflex from ligamentum patellæ lost. No paresis.

About two years after this first examination, Mr. A. died of a combination of renal and cardiac diseases. His spinal symptons had remained unchanged ; no ataxia at any time.

The diagnosis of posterior spinal sclerosis was verified in the post mortem examination of the spinal cord ; a distinct sclerosis of the lateral parts of the posterior columns was found.

Prolonged neuralgic stage (12 *years*); *early arthropathies.*

Case—5. A male patient sent to my clinic in 1877(?) by Dr. C. Williams. Has had characteristic pains in the legs for 12 years ; spots of pain hyperæsthetic at time of attack. Slight numbness of feet ; swelling in both knees in last two years ; no difficulty in locomotion.

Examination shows moderate anæsthesia and analgesia in feet and legs to knees ; absence of patellar tendon reflex. Chronic arthritis of both knee-joints with crepitations. Careful tests with eyes open and closed revealed no staggering or ataxia.

Prolonged first stage; development of general paralysis of the insane before ataxia.

Case 6. An ex-army officer seen in July 1878. He was then 35 years of age and apparently in good health. Was much exposed during the war in 1864–5, and in the latter year had typho-malarial fever. Then enjoyed better health for several years. In 1876, while on duty in the far West, had two attacks of "cerebral congestion," in one of which he fell unconscious. This was followed by poor health, depression, and hypochondriasis ; was almost insane on the subject of abuse by brother officers, official neglect, etc. Left the army and seemed well again, though a strong feeling of professional disappointment was prominent in his mental state.

Mr. B. consulted me for a peculiar "neuralgia." As far back as 1872 he had had attacks of cutaneous pain, becoming more and more frequent and severe. Attacks irregular as to time and location, mostly below the waist ; a few lately in the arms. Pain is sharp, cutting, and tearing, affecting round or oval areas in the skin and subjacent muscles ; not at all in the course of nerve-trunks. The pains were at times extremely severe, and their seat was nearly always hyperæsthetic.

Has had no numbness, vesical or optic symptoms. No sexual excitement.

Examination showed absence of patellar tendon reflex, and a little staggering when standing with eyes closed.

In 1881 I met Mr. B., but under such circumstances that I could not examine him. I was able to observe, however, that his pupils were extremely small. He seemed to stand well during the conversation.

During 1883–4 the case rapidly developed into a very well-marked one of general paralysis, with exaltation. The following details were obtained from Mrs. B.:

> Excepting for attacks of fulgurating pains Mr. B—— seemed fairly well until 1882. During the winter of 1882–3 he was often depressed, and was oftentimes found weeping and sobbing in his library. At other times was bright, hopeful, and even "high" without actual delusions. Of these abnormal psychic states the depressed periods were longer and more marked. During the summer of 1883 delusions and exaltation appeared. At times considered his wife insane; at others blamed her for his illness, because of refusal to have complete sexual intercourse. After he was placed (July, 1883) in the private institution where he now lives, he declared that his wife had feigned his insanity to get him incarcerated! Letters written in November, December, 1883, and January, 1884, are typical of general paresis in composition and mechanical execution. I saw him in February and he remembered me perfectly, as also my diagnosis of posterior sclerosis. He laughed at this, and stiffened out his leg and arm, and strutted about to show me how absolutely free from ataxia he was. He appeared prematurely aged; his speech was quick and quivering; his facial and lingual muscles showed fibrillary tremors. His walk was slightly ataxic, and there was no patellar reflex. The pupils were unequal in size and did not respond to light and shadow. During the past spring the patient had two epileptoid attacks, with semi-coma. This was followed by temporary right hemiplegia and aphasia. Later he was reported as quiet, childish, and full of exalted notions.

From 1872 to 1882, a period of eleven years, the chief symptoms in this interesting case were the fulgurating pains, absence of tendon reflex, and fixed pupils (?). The ataxia observed during the present year is hardly as distinct as we see it in ordinary tabes, and the walk resembles that of general paretics.

Cases of tabes terminating with symptoms of general paralysis are not very rare, but in all I have seen, the spinal symptoms were complete and advanced before signs of cerebral degeneration showed themselves. In the case related it seemed as if the dependence between the cerebral and the spinal lesion was doubtful. From what we know of the pathological anatomy of the two affections, it would seem that Mr. B—— had only a slight sclerosis of the columns of Burdach (the posterior radicular zones) at the same time that his cortex cerebri was the seat of advanced and rapidly progressing inflammatory action.

EDITORIAL.

NOTES OF A VISIT TO EUROPEAN MEDICAL CENTRES.

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Not one of the European medical centres can compare with Berlin in the progress which has been made in the past decade. The new buildings of the chemical, physical, physiological, and pharmacological Institutes, the Surgical Polyclinic on Ziegel Strasse, the Woman's Hospital, and the pathological Institute of the Veterinary College, all beautifully built and splendidly—aye, elaborately—equipped, fill one with astonishment, and Yorick's phrase rises to the lips modified to : "They order this matter [of medical education] better in Germany." In addition, the city hospitals, at the Moabit and Friedrichshain are important new clinical laboratories, which supplement the Charité and the smaller institutions, such as the Bethausen, the Augusta, and the Jewish Hospitals. There may be disadvantages in the paternal form of government under which our German colleagues live, but these are not evident in a survey of their university and hospital arrangements. Could the figures be obtained, it would be interesting to compare the total expenditure upon these in Berlin during the past ten years with that of London or New York. The conditions, of course, are not properly comparable, as private enterprise can scarcely enter into competition with lavish government expenditure. The progress so striking in medical institutions has been in keeping with the rapid development of the city since the consolidation of the empire. From a dirty, ill-drained, mal-odorous, second-class capital it has changed to a bright, well-drained, bustling metropolis. Intellectually, too, it now holds the same position to Germany as London and Paris do to England and France. True, Leipzig and Münich are still formidable and successful rivals in certain departments. The medical societies are not excelled by those of any European capital in the number and importance of their scientific communications. In former days Virchow's voice was heard occasionally at the *Académie de Médecine* of Paris telling of some brilliant pathological discovery; now Koch stays at home and expounds his discoveries in anthrax, tubercle, or cholera at the *Berliner medicinische Gesellschaft.* The number of students has steadily increased, and now reaches, in the medical faculty, about 900. Foreigners do not frequent the classes in such numbers as in Vienna. The system of short practical courses, specially designed for them, does not exist, but the "*fereien*" courses, in March and September, are attracting many English and Americans, and are becoming very popular. Many of the classes mentioned in the calendar after the names of docents and extraordinary professors are not given unless a sufficient number of students are forthcoming; and, as this is not always the case, it not infrequently happens that foreigners, attracted perhaps by some special courses which they see announced, are disappointed and dissatisfied.

The Pathological Institute is but little altered, and it was very pleasant to sit in the same seat and attend again those celebrated demonstrations on morbid anatomy, the remembrance of which since 1873 had remained as an incentive in humbly following in a similar line of teaching. The splendid example set by Professor Virchow and his pupils in the manner of teaching morbid anatomy and the general technics of autopsies has been badly followed by British and American schools. For any one imbued with the spirit of Virchow's methods there is nothing more painful than to witness the ordinary hospital post-mortem, done perhaps by a junior assistant. The subject is one far too much neglected in the average college course in this country. Too often a student completes his curriculum without the opportunity of making an autopsy, and without any instruction in details or methods, and as to a practical knowledge of the morbid appearances of organs, he is lucky if he has had a chance to feel a pneumonic lung or see a fatty liver. And yet, within a few months of the obtaining of his doctorate, he may be called upon in a coroner's court to give important evidence requiring a fair amount of pathological information. It is not, of course, practicable in all our schools to have well-equipped pathological laboratories, and in many good institutions the number of post-mortems annually available is small —not perhaps more than one hundred, but even with this number regular and systematic instruction can be given in the methods of performing autopsies; while a weekly demonstration on the fresh specimens, with "pickles" from the museum, will form a class at once practical and popular. At the end of two such courses a man will go up for his doctor's degree knowing the difference between hypostasis and congestion, and in exposing the calvarium will not begin his incision in the supra-orbital region. To their great loss, English and American students at Berlin do not attend this class in any number. As a teacher, Professor Virchow's vigor is unabated, and the care and thoroughness with which this great master performs an autopsy before the class is a lesson to be laid to heart. The laboratory at the City Hospital, Friedrichshain, under Dr. Friedländer, also offers unusual pathological advantages. The amount of material is as large as at the Charité, but no special instruction is given. The facilities for the study of the morbid anatomy of the domestic animals have greatly improved within the past few years. The new Pathological Institute of the Veterinary College is admirably arranged, and so far as horses and small animals are concerned, the material is abundant. The abattoir offers a very extensive field of observation, and one which can be freely utilized, owing to the kindness of Dr. Hertwig, the Veterinary Superintendent, and Herr Dunckler, the Pathologist. Except the contagious diseases, which, of course, are rigidly excluded, examples of all the ordinary diseases of cattle, sheep, and swine can be readily seen. It may be safely affirmed that nowhere can parasitic affections be more easily studied; but more on this subject when I refer to the modes of inspection carried on at this institution.

The special difference between the pathology of to-day in Berlin, and of ten years ago, is the extraordinary development of the experimental and culture methods applied to micro-organisms as the agents in the causation of disease. In this direction the Pathological Institute has been thrown somewhat in the shade by the *Reichsgesundheitsamt*, where Dr. Koch has done so much good work. One is startled at the rapid diffusion of a knowledge of these matters among the laity, explicable, no doubt, by the presence last year of the Hygiene Exhibition, and the demonstrations and lectures then given on the subject of germs and their development. The properties of various bacilli form subjects for table talk and, naturally, the amount of nonsense and pseudo-science which prevails is what might be expected. Everywhere the culture apparatus is seen, and it is rare to meet an assistant who has not got something under cultivation. There are no sceptics, at least, if there are any, they are "lying low" for the present. There were even murmurs against Virchow when, in the Reichstag, speaking on the cholera bacillus, he warned the members not to think the question finally settled with its discovery, and hinted that we were no nearer the eradication of consumption with all our knowledge of tubercle bacillus.

The relation of micro-organisms to anthrax and tubercle is accepted as quite settled, but the discussion wages hotly about pneumonia, diphtheria, puerperal fever, and cholera. On the last-named disease, the recently-published communications of the Berlin Conference have given much fuller and later details than were at my disposal. There can be no doubt of the constant presence of a peculiar bacillus-like organism, but of its precise relationship there will be for some time differences of opinion. Dr. Koch's communication will carry conviction to the minds of many; those who know him best and who know his conscientious and painstaking mode of working will accept his conclusions without hesitation. The magnificent ovation which he received on his return from India, and the handsome allowance granted by the Government were tributes to honest work and modest worth.

The attitude of many prominent men at the discussion on pneumonia, at the German Medical Congress, showed the temper of the times. There was an evident disposition to accept as specific the micrococcus which has been found in the exudation and sputum. Professor Jürgensen, who opened the debate, was most positive in his statements and treated the question as if practically settled. The facts of the case are briefly these: numerous micrococci are in the exudate within the air-cells in the sputum, but are not discoverable at all stages. An investing capsule was thought, at first, by Dr. Friedländer, to be a distinguishing feature, but it has been shown to be present in some cocci from the mouth. It is capable of cultivation on gelatine, and the form of growth is somewhat peculiar, possibly distinctive. Inoculation with the cultures, or the fresh exudate, induces a fatal disease in mice and guinea-pigs, characterized by serous inflammations and patches of lung consolidation, which is regarded as true pneumonia. Whether these cocci are peculiar to the disease or whether they are only the normal buccal and respiratory micrococci which have found in the exudate a suitable nidus for development, and whether the disease induced in mice and guinea-pigs is any thing more than a septicæmia, are questions which remain to be settled. Dr. Klein has written strongly in favor of this view. The micrococci are often difficult to demonstrate. The method recommended by Friedländer, and which gives good results in many cases, is that of Gramm (methyl-violet and iodine solution), but even when it fails we must not conclude that all cocci are absent. Professor Weigert has, on several occasions, demonstrated to me numerous cocci in the exudate cells in pneumonic lungs by a method which he has not yet published, when the ordinary methods proved quite futile. The communication of Emmerich is a remarkable one; he found in the stuffing between the floors of the dormitories of the prison at Amberg a micrococcus which he believed had been the cause of the repeated out-breaks of pneumonia in the institution at intervals during the past twenty-five years. He cultivated it and produced a disease in animals similar to that described by Friedländer. Sand and mortar, the composition of the stuffing, do not seem to offer the most favorable nidus for the maintenance of micrococci, but in this instance even such a barren soil is referred to as crowded with colonies of these tenacious organisms. We want more light on such cases.

In spite of the amount of labor spent on the micro-organism of diphtheria, we seem still far from full and accurate knowledge of their characters and relations to this terrible affection. Dr. Löffler, one of Koch's assistants, has been doing a good bit of work on the subject. Among a multitude of bacteria and micrococci which are found in the membrane and adjacent tissues, two forms appear to have a possible connection with the disease—a chain-like micrococcus and a rod-shaped microbe with an enlarged end. Cultivation of the former and inoculation of it into animals kills them, and similar bodies are found in the internal organs; but there does not appear to be the slightest connection between the affection thus induced and diphtheria. On the other hand, cultures of the rod-shaped organism proved fatal with production of diphtheritic membrane at the site of inoculation, swollen glands, serous, and hæmorrhagic effusions, etc., and extensive diffusion of the microbes throughout the body. Curiously, these cannot be again cultivated—a very weak point in the evidence. The membrane which is produced by the inoculation of the rod-like body is practically identical with that of diphtheria. Dr. Löffler's specimens and animals were particularly interesting, and we may anticipate good results from the continuance of his investigations.

So also with puerperal fever. We seem still far from the discovery of the true contagium. Chain-like cocci have been found by many observers in many cases in exudation, the blood, and the solid organs; indeed, they seem to be the only forms of any significance which have been constantly met with. They are not distinguishable from the similar forms which occur in scarlet-fever, erysipelas and diphtheria. Pasteur and others have cultivated this microbe, as obtained from the blood during life, but without leading to any very definite results. A series of investigations is being conducted on the subject by Dr. Tomer of the University Woman's Hospital, in conjunction with Dr. Iovanovic of the *Reichsgesundheitsamt.* In one case of well-marked puerperal fever blood taken with every necessary precaution was found to contain the micrococci, and was cultivated on gelatine to the eighth generation. The results of the inoculation with the cultures thus obtained are, as Fränkel's observations show, very uncertain. The present state of our knowledge, while not enabling us to speak with any degree of certainty, justifies the anticipation that ere long more positive information will be forthcoming—information which, at any rate will throw light on the vexed question of the relation of puerperal fever to diphtheria, scarlet-fever, and erysipelas.

It is a curious circumstance that Actinomycosis, known in Germany since the publication of James Israel's case in 1876 and Bollinger's article in 1877, should, so far as human cases are concerned, be confined to the country of its discovery. So far, no instance has been recorded in England or America. It is stated that the case of Mr. Treves of London, did not turn out to be genuine. In France they have only just woke up to the existence of the affection, and the article in the *Revue de Médecine* for April last is quite exhaustive. No doubt careful observation will reveal cases in this country, where the disease prevails rather extensively in cattle, more so, indeed, than among German stock, judging from a somewhat limited observation of the latter. Being familiar with the disease in cattle it was interesting to have an opportunity of seeing a case in the human subject. A woman about 30 years of age was admitted into the Charité on March 1st suffering from abscesses in various regions ; one over the sternum very soon burst and formed an extensive ulcer. After about two months' residence, and a course of illness resembling chronic pyæmia, she died in collapse. The pus was examined during life, but no diagnosis was made ; it was thought possibly to be malignant pustule. The autopsy revealed numerous abscesses about the size of a walnut in the subcutaneous tissue of various parts, some of them large and confluent, necrosis of the sternum and costal cartilages and perforation of the pericardium and thickening with union of the layers. Small abscesses in right ventricle, in the kidneys and liver, and in some of the bones. In the intestines there were ulcers and small abscesses, and in the colon a sort of diphtheritic process. In the pus from these widely-distributed abscesses were found the small grayish-yellow nodular bodies characteristic of the actinomyces. These can be seen readily with the naked eye and are quite distinctive—round in form and half the size of the head of a small pin. These granular bodies are usually more or less calcified, and the pus when rubbed between the fingers may feel quite gritty. In this case the actinomyces granules were softer, but each contained the well-defined radially arranged tufts of this most remarkable fungus. Chronic abscesses with bone disease have been the prevailing features of the majority of recorded cases of actinomycosis—twenty or thirty in number. This one was peculiar in the extent and character of the affection of the bowels ; the ulcers of the small intestines seemed directly due to the bursting of small abscesses, and the diphtheritic process in the colon appeared consequent upon the presence of the actinomyces in the deeper parts of the wall of the intestine. The teeth were carious, but there was no evidence to be obtained as to the origin of the trouble or the point of entrance of the fungus. Dr. Oscar Israel has cultivated the fungus, but there are still points in its biology which need clearing up. It is not a little remarkable that the affection should differ so in cattle and in man. In the former it is almost invariably local, confined to the jaw (lower), the tongue, or the pharynx, rarely occurring in the internal organs and not always producing active suppuration. Indeed, the majority of actinomycotic jaws which one examines present no suppuration. In man, on the other hand, the cases resemble chronic pyæmia, there is bone disease with multiple abscesses, and there is wide-spread generalization, as in the case here mentioned.

Additional interest has been lent this subject by the alleged discovery of actinomycosis in swine—in the muscle, not in the jaw or bones. The fungus lies within the sarcolemma, and produces degeneration of the muscle substance, but no suppuration. The degenerated fibres can be seen as opaque white streaks among the normal ones. The observation of Herr Dunckler, at the Berlin abattoir, has been confirmed by Prof. Virchow and Dr. O. Israel, rather prematurely in the opinion of many competent observers, who deny that the appearances in question are in any way produced by a fungus. A careful study of many specimens has not been fully convincing, but of this more on another occasion and in another place.

Professor Goltz and his dogs excited no little attention in April, when his demonstration at the Medical Congress aroused the partisans on either side, and the question of cerebral localization for the moment became all-important. How old the question now seems! and yet it was but at the time of my last visit that Fritsch and Hitzig were making their famous observations. "Knowledge grows but wisdom lingers" may be well said of this subject, about which we have learned so much in the past decade, and yet seem to have so little of that *wisdom*, or full truth, which is above the reach of controversy. The dog which Goltz brought from Strasburg, was stated by him to have had removed the entire motor zone, as understood by Fritsch and Hitzig, at two operations, Oct. 10 and Nov. 18, 1883. The dog, to a superficial observer, seemed to behave in a perfectly natural manner, and was not paralyzed either as regards motion or sensation. The animal took food well, was friendly, gave the paw, and was quite intelligent. The sensation of the feet appeared to be a little dulled. Similar results always followed this operation, and Professor Goltz regarded the very foundations of the localization theory shaken by such experiments. Although not paralyzed, Goltz observed that animals from which large sections of the anterior part of the brain had been removed, presented a curious motor disturbance—a loss of delicacy and aptness in the movements. In gnawing a bone they did not hold it deftly with the foot, but there was a very evident clumsiness, and the same was most marked in their attempts to take a bit of meat held up before the mouth—several trials being needed for a successful seizure. There were one or two curious reflexes in these animals—thus, when the root of tail was rubbed, the tongue was protruded ; and when the head was stroked, the animal would push the head against the hand with force, and even make rotatory movements. Another curious circumstance was the alteration in disposition consequent upon removal of the anterior parts of the brain. The dogs became ill-tempered, irritable, and in marked cases the condition resembled rabies. This was in striking contrast to the state of animals from whom extensive portions of the hinder part of the brain has been removed, as they become, even if previously cross and savage, quite good-tempered and confiding, as if the organ of mistrust and suspicion had been removed. The dog was killed the following morning at the Physiological Institute, and its brain given to Prof. Fritsch for examination, and as in the case of the dog shown at the London Congress, the destruction was not nearly so extensive as supposed. Goltz stated that "the gyrus sigmoideus to the level of the corpus callosum had been removed," and that the sulcus cruciatus and the neighboring parts would be found wanting. It was certainly a surprise to many to find how comparatively limited the lesion was ; in fact, the dog retained by far the larger part of the motor area on both sides. At a meeting of the *Verrein für innere Medicin* some weeks later, Prof. Fritsch showed the hardened brain on which the limitation of the lesions could be more accurately traced. Figures will no doubt be published, and will show that this dog, like the London one, has not been very favorable to its master's views.

Berlin possesses a model cattle market and abattoir, which presents many interesting aspects for the sanitarian, inasmuch as the questions of disinfection, inspection, and the like, appear here to have received unusual attention. It occupies a district of many acres at the eastern border of the city, with special railway facilities and ample accommodation for both market and slaughtering purposes. The total expenditure has been about two and a half millions of dollars. Of the cattle market we need not speak, although the general arrangement and the modes of disinfection of cars, stalls, etc., are important sanitary questions. The abattoir is for the city and district, and private slaughtering is rigidly forbidden. With the exception of swine, the animals are killed in small stalls, not in large abattoir halls, as is customary here. This was a concession to the butchers, who can in this way have their private stalls. For purposes of inspection there is a staff of 141 persons, viz.: the Veterinary Superintendent, Dr. Hertwig, eleven Veterinary Surgeons, eight Inspectors of the trichinæ department, eighty-six microscopic examiners, thirty sample procurers, and four stampers. The inspection is both ante- and post-mortem. The former takes place in the stalls of the abattoir, and any condemned animals are handed over to the veterinary police. The post-mortem examination of cattle and sheep is entirely macroscopic and easily performed. Every carcass is thoroughly inspected within a few minutes after death, when, if healthy, it receives the proper stamp; but if any parts are diseased, as liver or lungs, they are confiscated. The extraordinary number of 128 persons are occupied in the examination of swine, chiefly in the microscopic inspection for trichinæ. The coarse inspection is performed by veterinary surgeons, and is directed very largely to the detection of cysticerci (measles), tubercle, etc. Each "sample-taker" removes four pieces of muscle from each animal—from the rectus, abdomenalis, the diaphragm, the laryngeal muscles, and the intercostal—which are placed in a little numbered box, the number of which is stamped on the hog and entered in a book. When ten or twelve little boxes are filled, they are taken to the microscope room and given in charge of one of the inspectors. This department is most interesting, and constitutes an histological laboratory with eighty-six workers, half of whom go on duty in the morning, and the other half in the afternoon. Four of the inspectors direct each division. These workers form part of a large army of persons who are engaged in Germany in the examination of swine-flesh for trichinæ. To qualify for the position a person must receive a certain number of lessons in the microscope and in the examination of trichinous flesh, and pass the prescribed examination. Each one provides himself—or herself, for there are many women engaged in the work—with a microscope, which must have a magnifying power of fifty or sixty diameters. The little boxes with bits of muscle are distributed among the workers, and from each bit six small portions must be teased up and examined microscopically. A great saving of labor is effected by the employment of a large plate-glass slide, 21.5 cm. by 4.5 cm., which is ruled into twenty-four compartments, on each of which a small bit of muscle is spread. A similar-sized plate of glass fits upon this, and is screwed down, so that by pressure the bits of muscle are well flattened, and any trichinæ which may be present are readily seen. If detected, the animal from which the muscle has been obtained is at once known, as a strict record is kept, and the carcass is confiscated and destroyed.

The following figures will give an idea of the value of the inspection. For the six months ending 30th September, 1883, there were killed (about) 44,000 cattle, 42,000 calves, 94,000 sheep, and 103,000 swine, in all 278,000 animals. Of these, 79 oxen, 9 calves, 6 sheep, and 697 hogs were confiscated. Of the total number there were found tuberculous, 681 oxen, 2 calves, and 765 hogs, and on this account 67 oxen, 2 calves, and 46 hogs were totally confiscated and 2,898 single organs. It is only when an animal is extensively diseased and emaciated that the entire carcass is destroyed—otherwise the tuberculous organs are alone confiscated. The great prevalence of tuberculosis in swine is noteworthy; they appear in Germany to be more often affected than cattle. For the presence of cysticeri or measles 488 swine were confiscated—or rather boiled down for fat. There were 121 trichinous hogs—rather more than 1 per 1,000—two of which were also "measled" and several times a small distome was found in the muscle. Thus the 128 workers in this department during the six months found 121 trichinous hogs. It seems a small result for the enormous amount of time and labor expended, to say nothing of the cost of the inspection, but we must consider the other side of the question. A single badly-infected hog may, if it escapes detection, spread disease and death broadcast in a community given to raw *Schinken* and *Wurst*. The recent epidemic at Emmersleben illustrates this. The town and district had 760 inhabitants with one butcher and an old barber for meat inspector. The custom of eating raw chopped pork on bread was very prevalent, and on the 14th and 15th of September last a large amount of this was distributed among the villagers with the result in a few weeks of producing 257 cases of Trichinosis with fifty deaths. Even with inspection there is danger, particularly in country districts where the examination is liable to be careless, and where an inspector must possess unusual patience to examine, with perhaps negative results, several hundreds of animals, and keep up the necessary degree of watchfulness. The only safeguard is in the total disuse of raw pork, but it will take many years to educate Germans to this point. There can be but little doubt that the immunity enjoyed by the people of the United States is due to the habit of thoroughly cooking pork which so generally prevails. Whether American swine are more often trichinous than the German is a question which has received very contradictory answers—answers which in some instances appear to have been given largely on commercial or national grounds. Virchow has stated that the dangers are greater from native than from American pork ; the latter is of course smoked and pickled, but the records of inspection at Hamburg and Havre show, beyond a doubt, the great prevalence of trichinæ in the pork exported from this country. From 1878–1883 there were 335,840 inspections at Hamburg of American hams and sides, of which 3,470 were affected, and in the same period 304,725 inspections of native animals, hams and sides, of which only eight were trichinous. And when we remember that time and again animals have been infected when fed with pickled or smoked trichinous flesh we are less surprised at Bismarck's vexatious restrictions and at the outcry in Germany at the laxity of the United States government regulations regarding the inspection of swine.

The two city hospitals are well ordered institutions, presenting in many respects an interesting contrast. That at Friedrichshain, completed in 1874, is both medical and surgical, and has accommodation for six or seven hundred patients. Dr. Riess has charge of the medical wards and Dr. Hahn of the surgical. The situation of the hospital is magnificent—in the midst of a beautiful park, and the pavilions are far apart and surrounded by trees and shrubs. The buildings are very substantial, brick, faced with stone ; the medical portions of two stories, the surgical of one.

The abundant clinical material is not utilized for teaching purposes but visitors are warmly welcomed by the courteous directors, and a morning round on either side shows an extraordinary variety of instructive cases. The Moabit Hospital, under the charge of Dr. Paul Guttmann, consists of twenty-four pavilions of the simplest possible type. Each consists of a single ward with thirty beds, a small pantry, a nurse's room, and a waiting room. The construction is extremely plain—coarse brick walls and a thin board roof, covered with tar cement. There are large double doors at either end which can be opened wide for ventilation. The heating is by steam from a central building. The cost of each was about 3,000 marks. This hospital is largely for epidemic and contagious diseases, and has no surgical wards. The patients are drawn from the lowest classes in the city, and are often brought to the institution in such an enfeebled state that treatment is unavailing. The mortality is in consequence very high. Of 2,599 patients treated last year 566 died. On going through the Berlin hospitals one is struck by the appearance of the inmates who seem drawn from more impoverished classes than with us.

The custom of placing one or two men in charge of a large hospital seems odd to us and has both advantages and disadvantages. Thus, Dr. Guttmann is responsible to the city authorities for the care of about 350 patients at the Moabit institution and is, of course, allowed a staff of assistants on whom necessarily a large proportion of the work falls, and in some cases the treatment is entirely in their hands. At the city hospitals the rotation of assistants is much more rapid than at the University clinics, where they gladly remain for years at small salaries for the sake of the opportunity of making reputations as clinical workers. At the Charité the wards of Frerichs, Leyden, and Westphal are clinical laboratories utilized for the scientific study and treatment of disease, and the assistants, under the direction of the Professor, carry on investigations and aid in the instruction. The advanced position of German medicine and the reputation of the schools as teaching centres are largely fruits of this system.

NEW BOOKS AND INSTRUMENTS.

A Practical Treatise on Disease in Children. EUSTACE SMITH, M.D., Physician to East London Children's Hospital and to the Victoria Park Hospital for Diseases of the Chest. New York: William Wood & Co., 1884, pp. 884.

In this volume Dr. Eustace Smith, already so favorably known by his monographs on the wasting diseases of children, has given the profession an excellent treatise, and one by no means superfluous in English literature. For, although the main outlines of the subject are necessarily the same as in several other well-known systematic treatises, yet they have been modified by Dr. Smith in many details, so as to give a sufficiently individual stamp to even the didactic part of his own descriptions. These are, moreover, admirably illustrated by clinical histories which are extremely well selected as types, not of routine cases of disease, but of the more puzzling combinations of symptoms for which the less experienced practitioner, especially, requires an authoritative precedent.

The classification of subjects in a systematic treatise cannot fail to indicate the author's views on several points of doctrine. It is interesting to compare the classification adopted by Dr. Smith with those of Meigs and Pepper (7th ed., 1882), Henoch (1881), and West (7th ed., 1884), who profess to make none. Thus, under acute infectious diseases, Dr. Smith includes pertussis, erysipelas, and cerebro-spinal fever. Meigs and Pepper define the same group as "general diseases resulting from special morbid agents operating from without," yet exclude from it pertussis, which is placed among respiratory diseases. West assigns a chapter to fevers, which includes typhoid, variola, measles, and scarlatina, but excludes every thing else, while not only pertussis but diphtheria are referred to respiratory diseases. Finally, Henoch, who has special and valuable chapters on diseases of the new-born and of the lactation period, omitted in the other treatises, refers to them descriptions of erysipelas and syphilis, and again places pertussis among respiratory diseases, although distinctly recognizing that it rather belongs to the infectious group.

Dr. Smith's second chapter contains a singular grouping. Under the title "General Diseases not Infectious," he classes together rickets, ague, rheumatism, and gangrene. The usual place for rickets, among diathetic diseases, seems much more correct. Ague deserves to be classed etiologically among diseases due to specific morbid poisons; and if the classification of gangrene is difficult, the difficulty is certainly not simplified by associating this singular affection with rickets and rheumatism. Dr. Smith is the only one of the four systematic authors we have mentioned who devotes a special chapter to spontaneous gangrene which he apparently identifies with Raynaud's "asphyxia of the extremities."

The lesion thus described, however is not limited to the extremities of the limbs, but may occur in scattered spots on any part of the surface of the skin, and is especially frequent on the genital organs. Gangrene of the mouth and lungs are considered apart. The occasional apparent development of pulmonary gangrene from a pneumonia, might seem at first to justify this separation. But the author points out that "in cases where lobar pneumonia ends in mortification of the lung, the gangrenous lesion cannot be looked upon as a natural consequence of the pulmonary affection. * * * The mortification of tissue is induced by something superadded to the original lesion," and this seems to be various conditions, embolic, or unknown, which will cause complete stasis in nutrient blood-vessels. By this, the proximate etiology is certainly assimilated to that of cutaneous gangrene, if, with the author, we accept the theory of Raynaud, that in "local asphyxia," there is a spasm of arterioles, followed by a migration of blood corpuscles and transudation into the skin. In cancrum oris again, the smaller blood-vessels of the diseased cheek are obliterated by coagulæ, and gangrene of the lungs and genitals frequently coincides. There is therefore no philosophic reason for assigning these three varieties of gangrene to as many different chapters. But for convenience of clinical reference, this sort of reduplication—in which the author skilfully avoids repetition—has considerable advantage, and several illustrations of the method occur in his book. It is remarkable, however, that Dr. Smith should admit that a nervous spasm of arterioles, constituting attacks of local asphyxia which last but a few hours, should ever be the cause of gangrene. "Children, the subjects of this tendency to spontaneous mortification, are liable to attacks of what has been called 'local asphyxia.' * * * These attacks do not always subside harmlessly. In some cases the symptoms grow slowly worse, and the affected part becomes gangrenous." Much more consistent with the character of the phenomena is West's statement, that gangrene, of any locality, is a "blood disease." It really bears much resemblance to one of Pasteur's artificial septic diseases—malignant œdema;—and it seems far from improbable that a special form of microbe will be found as the proximate cause of the neurosis in the human species.

Other unusual chapters in a treatise on children's diseases are those on leucocythæmia, lymphadenoma, enlargement of the spleen, anæmia, hæmophilia, scurvy, megrim, otitis and its consequences, spasmodic spinal paralysis, idiocy, fibroid induration of the lung, suppuration about the larynx, foreign bodies in the air-tubes, paroxysmal dyspnœa, ascites. Six chapters are devoted to diseases of the liver, and four to those of the kidney. Tuberculosis of the lung is treated with acute—*i. e.*, general tuberculosis, and a separate chapter consecrated to pulmonary phthisis. Dysentery is separated as a "specific disease," from non-specific simple inflammation, or entero-colitis. On the other hand, tabes mesenterica is merged in a general description of scrofula. "It is now known that the symptoms are due, not to the mesenteric swellings, but to the lesion of which they are the consequence, and that the caseous glands form a part, and often only a very insignificant part, of the disease." The author adds: "Of themselves they form a strong argument against the tubercular theory of scrofulous glandular enlargement: for caseation of the mesenteric glands, unless their size be such that they press upon neighboring parts, is in itself a by no means serious matter."

Bronchial asthma is described as one of the forms of paroxysmal dyspnœa, of which the others are stridulous laryngitis, pressure upon the trachea by swollen bronchial glands, and obstruction of a bronchus by a foreign body. True asthma is usually associated with emphysema, but may occasionally be a pure neurosis. The author notes as singular that this neurosis should be so rare in children, in whom other forms of nervous spasm are so common, "one of the peculiarities of early life."

These re-arrangements and partial reduplication of subjects, by consideration of them from more than one standpoint, help to render Dr. Smith's treatise usually full and complete for practical reference. There are, however, omissions. Thus, as already mentioned, there is no chapter on diseases of the new-born ; none on diseases of the nose ; and the chapters on both heart and skin diseases are extremely cursory. The section on nervous diseases contains no chapter on hysteria. A regretable defect in the book is the entire absence of precise bibliographical references. No array of authorities is anywhere attempted, and discussions on rival opinions, and on pathogenetic reasonings are either brief or altogether omitted. References, however, to recent observations and experiments, are frequent and apposite, yet lose much of their value because it is impossible for the student to verify the quotations, unless already familiar with the author from which they are made.

Among such pathological experiments may be cited those of Braidwood and Vacher, who examined the air expired by measles patients, by making them breathe through glass tubes coated with glycerine. The microscope afterward revealed in this glycerine numerous sparkling, colorless bodies, spherical and elongated. The same observers confirm the observations of Naumann, on the great proliferation of cells in the rete malpighii, extending along the hair and sweat ducts into the glands ; and, in addition, claim to find in the skin the same sparkling bodies discovered in the breath. Similar cell proliferations in the rete mucosum exist in scarlet-fever, so that both these eruptions are removed from the category of hyperæmias. In scarlatinous uræmia, the potash salts of the blood have been found increased to three times the natural proportion, and of this two thirds were contained in the serum, and not, as in healthy blood, in the red corpuscles.

The opening paragraph in the chapter on anemia, sums up the physiological conditions which predispose to the disease, from the researches of Davis, Poggiale, Wiskemann and others. "It appears that, in infancy, although the quantity of blood is greater than it is in mature life, in proportion to the entire weight of the body, this blood is of lesser specific gravity, contains more white corpuscles, less fibrine and soluble albumen, a smaller proportion of salts, and a considerably smaller quantity of hæmoglobine. With this comparatively dilute blood, the growing child has to undertake a larger work than is required from the adult. He has to supply material for growth and development ; * * * the heart is forced to greater efforts to drive a sufficient quantity of blood along the relatively wider arterial channels ; the lungs, to aërate the larger proportion of blood carried to them by a more capacious pulmonary artery. The lungs eliminate carbonic acid in far higher proportion than is the case in older persons. The amount of urea excreted by the kidneys is relatively much greater than it is in the adult. The work required from the different secretory and excretory organs, whose united labors go to build up the growing frame, may be judged from the fact that, within twelve months of its birth, the body has increased to three times its original weight."

This description is both concise and clear, but it seems to us to involve a certain confusion of ideas that is often met with on this same subject. The amount of hæmoglobine in the blood, *i. e.*, the amount of oxygen, does not require to be proportioned to processes of growth, but to those of function. When tissue-growth predominates over nervo-muscular function, the demand for oxygen diminishes. Thus, both in childhood and pregnancy the amount of oxygen is less, but the loss is not felt, for there is less functional demand to be supplied. Again, the wide arteries of infancy do not increase, but facilitate the work of the heart. And as the rôle of lung tissue in respiration is purely passive, it cannot be said to be "forced to greater effort," because more blood is brought to it by a wider pulmonary artery. Further, this relative activity of the pulmonary circulation, tends to diminish the dangers of pneumonia, even if it increase the frequency of the disease.

The chapter on scrofula, includes a brief description of spinal caries ; and might with advantage refer to other scrofulous diseases of the bones. The local consequences of glandular hypertrophy in the mediastinum are carefully described, and the various symptoms which result from their pressure upon blood-vessels and air passages. " It is possible that the bronchial glands may be occasionally the seat of tubercle, * * * but there is little doubt that the ordinary form of glandular enlargement is due to a very different cause " ; by which is meant the irritation of different lymphatics in subjects endowed with a " singular activity of all the epithelial structures." To this special activity the author attributes the luxuriance of the hair, eyebrows, and eyelashes ; the frequent scaliness of the skin, the rapid growth of nails, finally the peculiar tendency to engorgement of the lymphatic glands by proliferation of their elements.

The relations of scrofula and tuberculosis are touched upon lightly ; the influence of softening cheesy matter in setting up an infective process, alluded to, as also the (as yet unsettled) debate on the rôle of the tubercle bacillus. The author observes that the "tissues of scrofulous subjects seem to furnish a congenial soil for the growth of tubercle." It is certainly not premature to suggest that it is this very soil which is required for the development of the parasitic organism.

The diagnosis of acute tuberculosis in young children is carefully considered, and especial stress laid on the symptom of œdema of the extremities in the absence of albuminuria. "In young babies the only symptoms of the disease for a considerable time may be slight fever, pallor, some loss of flesh, an inelastic state of the skin, and a little œdema of the extremities."

The discussion on enlargement of the spleen from "simple hyperplasia" is a useful addition to the usual chapters on abdominal tumors, or to the usual incidental reference to the splenic enlargements which occur in the course of fevers, malaria, rickets, and syphilis. The writer points out that although the spleen may be enlarged in amyloid degeneration, it may become amyloid without being enlarged. A case is described in a child with slight rickets, where the lower border of the spleen was found to reach to the left crest of the ilium. We have seen a similar case in a child eight months old. In a child of a year, the spleen reached to the level of the navel, and there were some signs of rickets. After five months' treatment with cod-liver oil, fresh air, and nutritious diet, the child had cut ten teeth ; seven months later had cut sixteen teeth, and the spleen was only just perceptible below the ribs. It is advisable, observes Dr. Smith, not to give iodide of potassium, nor mercurials, except in cases of syphilis ; but to pay great attention to the nutriment and digestion, to protection from cold, and for medicine, to rely upon iron, cod-liver oil, quinine, and wine.

An interesting, and again an unusual chapter is devoted to scurvy. "As one of the diseases to which young children are liable, it has been, until recent times, completely ignored. Lately, however, owing to the observations of Drs. Cheadle, Geo. F. Barlow, and others, a form of the malady has been recognized as an occasional consequence in infants of bad feeding and injudicious management. In such subjects the disease is commonly grafted upon rickets ; and there can be little doubt that it is this conjunction of the two maladies which constitutes the state described by Fürst and others under the name of *acute rickets*."

That more ricketty children are not subject to scurvy is probably owing to the fact that many articles of food which preserve from scurvy actually favor rickets. The deterioration of the blood in scurvy is not caused by the mere absence of potash salts, but by the absence of those salts in combination with organic acids. The neutral salts, as the chlorides, are increased; the alkalies diminished; hence dissolution of blood corpuscles and fatty degeneration of the muscles and of the secreting cells of the liver and kidneys.

One of the most characteristic morbid changes in infantile scurvy is subperiosteal hemorrhage, a late symptom in the adult. Conversely, the affection of the gums usual in adults may be absent altogether in children. Thus the earliest symptoms consist in signs of pain and crying by the child when handled; then cylindrical swellings of the limbs; finally separation of the epiphyses occurs, and crepitus may be detected on passive motion. At this stage the patient, who hitherto has kept the limbs flexed, lies with them extended and motionless, and no longer suffers pain from handling. The principal difficulty in the diagnosis is the exclusion of syphilitic pseudo-paralysis. In the latter case, says Dr. Smith, the child suffers from "enlargement of the spleen and all the signs of a profound syphilitic cachexia. The child is greatly wasted; is hoarse, and snuffles; the cranial bones are thickened, and the skin has the peculiarly dry, parchment-like appearance. In scurvy the patients are not, as a rule, greatly emaciated. Often their general nutrition is fair, and the special characteristics of syphilis are absent. If the gums are spongy, or signs of hemorrhage can be noticed in the skin or elsewhere, the evidence is strongly in favor of scurvy."

We do not feel sure that all cases of syphilitic pseudo-paralysis do present such unmistakable evidences of cachexia. We have certainly seen several presenting the clinical features of the bone disease, accompanied by a specific eruption, and yielding rapidly to corrosive sublimate baths, in whom there were no cachectic symptoms at all.

Croupous pneumonia is regarded as a general disease with marked local manifestations, its exudation being peculiar to itself and not to be produced by ordinary inflammatory agency. It should be classed with tonsillitis; is favored by bad drainage; but cannot, with our present knowledge, be attributed to specific infection.

The prognosis of croupous pneumonia is declared to be always favorable—that of broncho-pneumonia always doubtful, a statement whose gloom seems to us decidedly exaggerated, except in regard to diphtheritic pneumonias.

The chapter on collapse of the lung is usefully divided between two special sections, congenital and post-natal atelectasis. Acute phthisis is separated from pulmonary tuberculosis, and the term "restricted to cases of rapid catarrhal pneumonia, when, as a result of an acute inflammatory process, the air-cells become stuffed with epithelial elements which undergo rapid caseation, and the solidified tissue quickly breaks down into cavities." The prognosis of *chronic* phthisis is not unfavorable in a child who can be put in the best sanitary conditions.

The chapter on jaundice, though principally occupied with jaundice of the new-born, contains no reference to Buhl's disease, or the acute fatty degeneration of the liver, kidneys, and blood-vessels in the foetus or new-born.

In the chapter on chronic Bright's disease is described a peculiar condition of "renal inadequacy," or diminished functional activity of the kidneys. "The disorder is indicated by pallor weakness, wasting, constipation, sometimes by sickness, and in every case by a remarkable absence of the natural elasticity of the skin. * * * There is no albuminuria, but the quantity of urine is small and the specific gravity low. Evidently sufficient solids are not discharged by the kidney, and the retention of effete matters in the system is apparently the cause of the symptoms." The average daily quantity of solids for a child should be five grains for every pound of body weight. A case is cited where the solids were no more than two and three quarter grains per pound. The remedy for such cases, which has proved successful, is brisk purgation, preferably by senna.

Directions for treatment are careful and precise throughout the treatise. Among these directions, however, we certainly should not agree to that on the quinine treatment of pneumonia. The author seems to speak less than usual from personal experience, for he says: "Quinine is strongly recommended by some authors as a valuable remedy * * * which is said to quickly reduce the temperature without weakening the heart * * * and for its supposed influence in checking the spread of the disease over the lung." The author advises "full doses," says that "children bear the remedy well," yet then recommends for an infant of twelve months only one grain three times a day, the quantity to be increased one grain and a half for every year of the child's life. These doses are really quite inadequate, and no judgment on the value of the treatment can be based on them. Dr. Smith also says that in case of great dyspnœa and threatened cardic failure from over-distension of the right heart, he "should not hesitate to take one, two, or more ounces of blood from the arm." But he does not as yet seem to have practised his own suggestion. It is asserted, assuredly wrongly, that local treatment of erysipelas is useless. Now we have records of several cases where the eruption was arrested by a band of carbolic-acid painting. Dr. Smith quotes a new method by Barwell, application of a thick coating of common house-paint, but does not seem to have tried it. One treatment is "invariably" used by the author in pertussis: sulphate of zinc ($\frac{1}{6}$, $\frac{1}{4}$, $\frac{1}{3}$ of a grain) and atropine ($\frac{1}{2}$ drop of solution P. B.) twice or thrice daily; the atropine to be gradually increased to dilatation of the pupil. Rheumatic pericarditis is treated by neither ice nor poultices, but by a blister and iodide of potassium, and great efficacy is attributed to the treatment. In the treatment of infantile syphilis, the use of sublimate baths is, we think unadvisedly, ranked far behind internal treatment by gray powder or the perchloride ($\frac{1}{52}$ to $\frac{1}{48}$ gr. at a dose). For anæmia, as in other cases indicating the use of iron, especially rickets, Dr. Smith prefers the acid salts as sulphate or perchloride to the iodide syrup. The latter, he says, often occasions gastric catarrh by promoting acidity and flatulence, and encouraging the excessive secretion of mucus. Unusually large doses of iron are recommended—thus, 5 grains of the exsiccated sulphate three times daily, or 20 to 30 drops of the perchloride as often to a child six years old. For purpura, in occurring suddenly in healthy children, the "old plan of energetic purgation is peculiarly valuable. Often in such cases a course of iron or other tonic is followed by no benefit whatever, while a few doses of some drastic aperient cause a prompt and final disappearance of all hæmorrhagic symptoms. * * * The best * * * is a combination of oil of turpentine with castor oil." For the treatment of megrim, Dr. Smith warmly recommends a combination of ergot and strychnia (10–15 drops of liquid extract of ergot with spirits of chloroform, and 2–3 drops of strychnia solution, P. B., three times a day). In most cases in young subjects, the attack itself may be "decidedly shortened by a dose (♏ xv–xx) of liquid extract of ergot with spirits of chloroform in camphor-water." The raw-meat treatment of inflammatory diarrhœa, formerly advised by Trousseau, is warmly endorsed by Dr. Smith. "At first it is only partially digested, and the decomposing residue gives a most offensive smell to the stools; but after a few days, especially if pepsin be taken, the meat soon ceases to be visible in the motions. By the above measures strictly carried out, the most obstinate cases can be arrested."

We might sum up this review with the observation, that Dr. Smith's systematic treatise is an excellent expansion of his earlier monographs, not profoundly analytic, but giving vivid, accurate, and sufficiently comprehensive clinical pictures of disease, interpreted without anacronisms, and from thoroughly modern standpoints. [M. P. J.]

Diseases of the Heart and Thoracic Aorta. By BYRON BRAMWELL, M.D., F.R.C.P.E., Lecturer on the Principles and Practice of Medicine, and on Practical Medicine and Medical Diagnosis, in the Extra-Academical School of Medicine, Edinburgh; Pathologist to the Edinburgh Royal Infirmary, etc. New York: D. Appleton & Co., 1884, pp. 784, with 317 Illustrations.

Dr. Bramwell is well known as one of the most energetic workers of the younger generation of physicians in Edinburgh, where he now holds—as he did at Newcastle-on-Tyne, his former home—positions in the Infirmary and extra-mural school, which ensure great clinical and pathological advantages. His work on the "Diseases of the Spinal Cord," published two years ago, was very well received, and has proved a most serviceable text-book for students. The present volume is, we are told in the preface, the outcome of fifteen years' study and observation on diseases of the heart, and the subject-matter of it was delivered, almost exactly as it stands, in the form of lectures to the author's class at the beginning of the winter session of 1883–84. The work bears many traces of its class-room origin, in the systematic arrangement, and in the innumerable—often tiresome—subdivisions of chapters and paragraphs. Here, as in the author's work on the "Cord," we are at once struck with the number and general excellence of the illustrations, in which respect it compares most favorably with all recent manuals on the heart. Dr. Bramwell is evidently a strong believer in the value of good figures, and of the whole number in the work more than half are original.

The preliminary anatomical and physiological observations are well up to date. Full use is made of Dr. Gaskell's work on the automatic mechanism of the heart, and of his suggestion that in man also it is possibly due to an inherent rhythmical property possessed by the muscle; and also of the recent contributions to cardiac physiology from the biological laboratory of Johns Hopkins University. This chapter will be read with great interest by many practitioners who have not ready means of access to recent literature upon these subjects. The author very properly insists, when speaking of the relation of the heart to the minute blood-vessels, upon the necessity of taking an *all round* view of the circulation, and not limiting our vision to the central propulsive organ.

Speaking of the dyspnœa, among the cardiac symptoms, the Cheyne-Stokes respiration is very fully discussed—perhaps too fully, as thirteen pages are devoted to it. The conditions under which it most commonly occurs are dilated and fatty right heart, atheroma of the coronary arteries and dilitation of the aorta, and the phenomena are probably to be explained by supposing that periodical variations occur in the amount of oxygen supplied to the medulla. The respiratory centre "is the seat of two conflicting forces: one tending to generate inspiratory impulses (the discharging portion), and the other offering resistance to the generation of these impulses (the restraining portion); the one or other alternately gaining the victory, and thus leading to a rythmical discharge." Venous blood excites the action of the discharging portion and depresses the action of the restraining portion, and arterial blood may be supposed to act in just the opposite way. The centre is moreover supposed to be in a state of irritable weakness. Now, to explain a paroxysm, we may suppose, starting at a period of apnœa, that the venous blood gradually excites the dyspnœa by arousing the action of the discharging portion, and in consequence of the excessive irritability of the centre the discharges become excessive, and a condition of dyspnœa is produced, but the centre becomes speedily exhausted and the dyspnœa subsides. By the rapid respiratory efforts the blood (previously venous) becomes arterialized, stimulation of the discharging portion of the centre ceases; stimulation of the restraining portion is produced and has full swing, inducing the condition of apnœa. Six diagrams are given representing the changes supposed to occur at the different periods of the Cheyne-Stokes cycle. Altogether Dr. Bramwell's discussion on this point, though long in proportion to its importance, is exceedingly interesting and suggestive.

In chapter III. excellent details for the student are given upon methods of case-taking, and there is a good summary of the chief symptons met with in heart disease. On the somewhat vexed point of the reduplication of the heart-sounds the author is, we believe, quite right in stating that the necessary conditions—in the first sound—are considerable asynchronism in the contraction of the two ventricles with diminished duration of one or other or both of the component parts of the reduplicated sound and a slow action of the heart.

One of the most unsettled questions in cardiac pathology relates to the mode of origin of the functional murmurs, and we find it here discussed at great length, twenty-one pages being occupied in its consideration. We have a most exhaustive analysis of the three views, viz. : (1) That the murmur is produced in the pulmonary artery ; (2) that it is due to mitral regurgitation, and (3) that it is caused by a constriction of the pulmonary artery by the dilated appendix of the left auricle. The arguments for and against the latter views are given in great detail, and the author concludes that the sudden propulsion of a large blood-wave of abnormal (spanæmic) composition into the pulmonary artery, which is possibly dilated, is the efficient cause ; at the same time he agrees with Dr. Balfour in thinking that in many cases of advanced chlorosis, the systolic murmur which is heard at the apex is mitral and due to regurgitation, but he also thinks that it is a distinct one from that heard in the second left interspace.

Pericarditis is the first disease considered ; the morbid anatomy is illustrated by eighteen figures, some of which are rather superfluous. Thus four plates are given showing the lymph and vegetations in the acute affection. Although a case is given, scarcely stress enough is laid on the occurrence of acute purulent (idiopathic) pericarditis, which is more common in children than is supposed. Among the accidental symptoms reference is made to the forms of mental derangement which occur in some cases, either as delirium, with the hyperpyrexia of rheumatic pericarditis, or as that peculiar mental derangement described by Sibson, Flint, and others—a taciturn melancholy with suicidal tendency, or in cases of hard drinkers it may assume the character of delirium tremens. Dr. Bramwell is very strong on tables of differential diagnosis, of which no fewer than seven are given in connection with pericarditis. We are sorry for the students in the medical class of the extra-mural school if they have the lively hatred of such tables which possessed the students of our time. Aspiration of the effusion is recommended when the action of the heart is seriously embarrassed, and in case of a purulent fluid the sac is to be laid open and a drainage-tube inserted.

The section on acute endocarditis—which is divided in acute and ulcerative forms—is well and clearly given. Ulcerative endocarditis is not thought to be a specific infectious disease like typhus. The clinical history is unusually full, and the types of *cardiac typhoid pyæmic* and *aguish* cases are recognized. The points of diagnosis are carefully given, and altogether it makes, perhaps, the best chapter on this disease in any English text-book. Under the treatment of endocarditis the question of the influence of the salicylates in rheumatism and upon the heart complications is fully entered into, and the author strongly advises the administration of full doses of pure salicin after the manner recommended by Dr. Maclagan, and thinks that "if this form of treatment were more vigorously carried out in the early stages of the attack, the frequency of endocarditis and other cardiac complications would be materially diminished."

The treatment of mitral regurgitant disease is divided into the periods of before and after the failure of compensation, and under the former, excellent general directions are given. The new remedies are not thought to be equal to digitalis in progressive cases. Arsenic is highly recommended as a cardiac tonic—one not enough used.

Chapter VI. is taken up with the consideration of acute and chronic endocarditis, hypertrophy and atrophy, and fatty degeneration. We were disappointed in not finding a section devoted to these not uncommon cases of hypertrophy and dilation, which are grouped under the unsatisfactory term—idiopathic. We want more light in the pathology of this class of heart cases. The patients are usually middle-aged men, powerfully built, often hard drinkers. They come under observation with all the signs of hypertrophy and dilation, the consequences of broken compensation, dyspnœa, and dropsy. After perhaps several such attacks, the patient finally succumbs, and on *post mortem* we find no valve disease and no renal affection, nothing, in fact, to account for the great hypertrophy and dilatation. We are accustomed to attribute the condition to hard work and alcohol, but how much is due to the one and how much to the other is very doubtful.

In connection with atrophy two plates are given of a very atrophic heart weighing only 1214 grs. (81 grammes), taken from a woman aged forty-five, dead of cancer.

In the chapter on cardiac neuroses, after speaking of neurotic palpitation, the important subject of angina is considered. Under the term *functional*, Dr. Bramwell includes a number of cases of heart-spasm and pain in young persons, under forty; it may be called pseudo-angina, and is not often fatal, but it may run into the more serious form. The pain is attributed to irritation of the sensory-nerve filaments in the heart walls; possibly due to spasm of the heart muscle—like the extreme pain of cramp. Possibly too, the nerves passing along the coronary arteries, which are so often found diseased, may be implicated, and Peter has found extensive changes in the branches of the cardiac plexus. There are no new suggestions on treatment.

In devoting only sixty out of seven hundred and eighty-four pages to aneurism and diseases of the aorta, we scarcely think our author allows a just proportion to such an important subject. Among the physical signs no mention is made of Surg.-Major Porter's valuable suggestion, that in cases of suspected aneurism a sensation of tugging at the trachea can be distinctly felt when, by elevating the larynx, the windpipe was put upon the stretch. The sign is only present when the tumor presses upon the trachea, but in some cases we have found it of great value. The remarks upon diagnosis are excellent. An extraordinarily rare and interesting case is mentioned of simple dynamic pulsation of the aorta, noted by the author and Dr. Murray of Newcastle. In the treatment of aneurism by iodide of potassium, the drug is stated to act by reducing the blood pressure and relieving the tension within the sac, not, as Dr. Balfour supposes, by acting on the fibroid and muscular elements of the sac. Small doses do as well as large ones. Chloral in 7-gr. doses three times a day is advised for the same purpose.

On the whole, the work is one which will prove of great value to students and practitioners, and Dr. Bramwell may be congratulated on the production of so excellent a text-book. [W. O.]

Diseases of the Throat and Nose. By MORELL MACKENZIE, M.D., London. Vol. ii., Diseases of Œsophagus, Nose, and Naso-Pharynx; with index of Authors and Formulæ for topical remedies. Illustrated. Philadelphia: P. Blakiston, Son, & Co., 1884, pp. 550.

In the preface we are told twelve years elapsed between the conception and birth of this book. If for no other reason than this assurance of its maturity, it is entitled to appear at a time when works in this specialty have become so frequent.

The gullet, the nose, diseases of the naso-pharynx, and an appendix on the contents, in as many sections. The anatomy of the part opens each of the first two sections, followed by a description of the methods of examination and of instruments, and the consideration of the various diseases by which the organ is affected. The author introduces each disease by a list of its Latin, French, German, and Italian names. Then follows a definition, which most often is not a definition, but a translation of the Latin term with a few conditions and sequelæ. This we acknowledge is unavoidable, if definition is in all cases to be insisted upon. In medicine, too little is known for purposes of definition, and in most cases the attempt to define should not be made. Each disease is illustrated by the record of one or more cases.

At rare points the author exhibits a conservatism, but nowhere more so than in his adherence to the term "thrush," refusing to recognize the very natural division suggested by the parasitic character of one class of cases of thrush, and the non-parasitic character of the other class. This distinction has long been made by the French.

Farther on, we confess no little surprise that a disease as dangerous as gummous syphilide of the gullet should have its treatment disposed of in a short paragraph and summed up in 10-gr. doses of iodide of potassium.

The operation of œsophagostomy and gastrostomy, which of late have received so great attention in surgical discussions, are considered to a full extent under "cicatricial stricture of the gullet." Œsophagostomy is characterized as having "a narrow range of usefulness," as "a leap in the dark," and more "likely to find favor with the adventurous surgeon than with the careful practitioner." It is, however, recommended in syphilitic stricture of the upper part of the gullet, but the "diagnosis of syphilitic disease can never amount to any thing more than conjecture." Gastrostomy is classed with œsophagostomy in the following: "It can hardly be denied, however, that the benefit of these operations has often been shown more in the euthanasia which they have brought about than in any appreciable prolongation of the patient's life. In fact, judging from statistics alone, operative interference would seem to be attended with less satisfactory results than the milder palliative measures generally adopted." Apology is offered for the bad showing in the case of gastrostomy, and the prediction is made that its future will be more satisfactory. A case of cicatricial stenosis with gastrostomy is given in full, with notes of the post-mortem examination.

In the treatment of diseases of the nose, the author agrees, with others, in not having observed injurious effects from irrigation, but prefers sprays. We hold the "word of caution" in the treatment of hypertrophy of the nasal mucous membrane to be most opportune. With the snare and cautery at hand, the temptation to operate seems to most, almost irresistible, where the author assures us that he succeeds "in effecting a cure by the simple removal of all causes of irritation and the persevering use of gentle dilatation." In the intractable cases, the electric cautery is recommended for the redundant tissue as "the most simple and efficacious."

Ozæna is given its right place. It is dethroned and given a place among the symptoms of a long list of different affections. The discussion of it comes here under the head of "Dry Catarrh" (which term the author defends against the etymological objection by Virchow). It may be observed that in considering the prognosis of dry catarrh, the contrast is drawn by implication between "dry catarrh" and "true ozæna."

Full cousideration is given of the more recent discoveries in the relation between nasal tumors and cough, asthma, epilepsy, etc. But, the author denies that, the disposition to hay-fever lies in chronic hypertrophy of the mucous membrane of the nose.

This volume is most valuable to the general practitioner, as well as to the special student, because of the excellent selection of its subject-matter, its arrangement, its extensive bibliography arranged at the foot of the page with reference from the text, its completeness, and the unobjectionable work of the publisher.

[J. V. D.]

The National Dispensatory. By ALFRED STILLÉ, M.D., LL.D., and JOHN M. MAISCH, Ph. D. Third edition, Henry C. Lea's Son & Co., Philadelphia 1884, pp. 1755.

After an interval of five years a new edition of this now indispensable work comes to us much improved. It contains seventy-five pages more than the second edition, but the increase in material is much greater because the size of the printed page has been increased by nearly half an inch each way. This change in size of page was doubtless made to prevent the book from acquiring unwieldy thickness, but it is unfortunate in that it interferes with the shelving of the work with other large octavos.

The additions consist in new articles, more recent quotations of physiological and therapeutical experiments, and the insertion of nearly one hundred more illustrations. The general index has been elaborated and contains nearly four thousand more references than the former one.

As now re-written, the "National Dispensatory" constitutes a formidable rival to the various treatises on therapeutics and materia medica, both for students and practitioners. [E. C. S.]

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ARCHIVES OF MEDICINE.

Original Articles.

ON THE RUPTURE OF VEINS, WITH THE REPORT OF A CASE IN WHICH THE RUPTURE OF A DEEP FEMORAL VEIN WAS SUCCESSFULLY TREATED BY LIGATION OF THE RUPTURED VESSEL.

BY HENRY B. SANDS, M.D.,
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THE accidental rupture of veins, if we except varicose veins that are superficial, is an occurrence which is either unmentioned or barely alluded to in most of our standard works on surgery; and the reason for the omission is obvious. As the accident is not a common one, and as it rarely proves fatal, its verification is difficult, and its pathology obscure. Nevertheless, an examination of the scattered literature of the subject teaches us that such lesions, although infrequent, are well authenticated; that the exciting causes are often trivial; and that the consequences are sometimes disastrous. The case I am about to relate shows that danger to life may be occasionally averted by operative interference.

The fact that the veins are liable to rupture did not escape the notice of the older pathologists, who have recorded some of the most remarkable examples of this lesion. Morgagni found a rupture of the azygous vein, the vessel being extremely varicose. Portal[1] describes a case of rupture of the superior vena cava, occurring in a woman who died suddenly in a cold bath. After death a rent was found in this vessel, just above the right auricle, a large quantity of blood having escaped into the right side of the chest. A similar case, in which the rupture took place into the pericardium, was observed by Helwin, and is recorded in Haller's "Physiology."[2] Andral[3] relates the case of a man in whom the inferior vena cava gave way during a violent struggle. The edges of the rent seemed as if they had been torn asunder, and the coats of the vein in the neighborhood were perfectly healthy. Rupture of the internal iliac vein has been known to occur spontaneously. The patient was a woman, apparently healthy, who was in the ninth month of her sixth pregnancy. She awoke one night with a violent pain in her right groin and hip. A physician was summoned immediately, but did not arrive until two hours later, when he found the woman in a state approaching collapse. Twenty minutes after his arrival she died, without any sign of labor. The body was examined by Sir William Blizzard on the following day, when the peritoneal sac was found distended with blood which had escaped from a rent in the internal iliac vein.[4] Hey, of Leeds, saw a child in whom, after a fit of screaming, a hæmatoma appeared over the external jugular vein. It was at first about the size of a pigeon's egg, but a fortnight later it had become very large, and extended from a point a little below the clavicle to the angle of the jaw. It was partly evacuated by repeated punctures with a couching-needle, and afterward gradually disappeared.[5]

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[1] "Anatomie Médicale," tome iii., p. 355.

[2] Vol. i., p. 129.

[3] "Pathological Anatomy," Am. translation, N. Y., 1832, vol. ii, p. 255.

[4] *London Medical Repository*, 1814, vol. i., p. 456.

[5] "Practical Observations in Surgery," by William Hey, F.R.S., second ed., London, 1810, p. 487.

The cases of greatest surgical interest are those in which the ruptured vein is situated in one of the extremities. In such instances some mechanical injury is the most frequent cause of the lesion. The axillary vein has been torn in an attempt to reduce an old dislocation of the humerus[1]; and most surgeons of experience have met with examples of simple fracture, accompanied with hemorrhagic extravasations of unusual amount, due, in all probability, to a rupture of some large vein. When death does not speedily follow the rupture, the lesion of the vessel is generally repaired, and the extravasated blood disappears by absorption or otherwise. But in rare instances the vein remains open, and a blood-cyst is formed, which may continue for a long period, as is shown by the following example:

"Michael Callahan, a mariner, was admitted into St. Thomas' Hospital on the 30th of January, 1766, for a large tumor on the inside of the right arm. He said that this tumor immediately followed a contusion he had received, with the inside of the upper part of his arm against the stock of an anchor, about four weeks before his admission into the hospital; that at first it was of considerable magnitude, but that afterward by some applications it was reduced to the size of a pigeon's egg. During two years it did not seem to grow any bigger, but afterward, upon his being attacked with a fever in the West Indies, from which he narrowly escaped, it gradually increased, and, when he came into the hospital, was considerably larger than a man's head, extending from the arm-pit, to within about two inches of the flexure of the cubit. The tumor was judged to contain blood; and this opinion was strengthened by the man's declaring that a surgeon, either at Portsmouth or Plymouth, had punctured it, and observing nothing to flow but blood, closed the wound, which soon healed up. On the 22d of March, 1766, he died. The next day the body was conveyed to the anatomical theatre; and, while we were placing it in a proper position, the tumor burst, and about a pint of serous fluid was discharged.

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[1] Froriep relates the case of a scrofulous patient, 26 years old, who was treated for a luxation of the humerus 20 days after the injury. On the second attempt, reduction was effected; accompanied, however, with an extravasation which proved fatal in less than two hours. Post-mortem examination showed that the axillary vein had been torn completely across. ("Veraltete Luxationen," Weimar, 1834, p. 35.) A similar case is recorded by Callender, "St. Bartholomew's Hospital Reports," vol. ii., 1866, p. 107.

"The first step taken was to open the thorax and pericardium, when all the large vessels springing from the basis of the heart were found, and of their usual size. They were traced to the neck; but they did not appear to be in any way diseased. The right axillary artery and vein were examined and found to be perfect. I next opened the tumor, and discovered it to be full of that coagulated mass which is always contained in old tumors formed by the extravasation of blood. I then began at the axilla to trace the artery, and found that it passed quite through the tumor, without any alteration with respect to its dimensions, and without any rupture or appearance of disease. The coagulated mass surrounded the artery intirely, but had not in the least corroded it, though it adhered closely in some places to its surface. I then intended to trace the axillary vein, and made a puncture into it, about an inch before it entered the tumor, into which I thrust a probe, and found that it passed easily on toward the mass. Upon putting my finger into the tumor I felt the naked extremity of the probe. I afterward laid bare the deep-seated branch of the basilic vein, which lies close to the brachial artery, just above the flexure of the cubit, and thrust a probe into it, which passed readily upward; and, upon introducing my finger again into the mass, I also felt the naked extremity of that probe. The parts remained in this state till the day following, that the physicians and surgeons of the hospital might see what had been done. When I came the next morning I found that the probe, which I had left in the axillary vein, had been pulled out, and I could not readily hit upon the orifice I had made for its introduction; I therefore made another, and, introducing a large blow-pipe, threw in air, and observed that the vein, just as it approached the tumor, was considerably dilated, and then opened into it. This likewise was seen by all the physicians and surgeons of the hospital."[1]

Often the violence that causes the rupture is slight and appears inadequate to explain it. Wise[2] relates the case of a man aged forty-eight, who, after a slight fall while walking in a dark night, was attacked on the following day with pain and swelling of the leg, which soon became much enlarged, the tumefaction extending from the knee to the ankle. An incision was followed by a gush of blood, partly coagulated, and a large cavity was found in the calf of the leg. A hospital surgeon was consulted, who, believing that an artery had ruptured, proposed amputation, which was declined. Subsequently the wound was enlarged, when it was ascertained that the extravasation was superficial, being situated beneath the skin. The clots were turned out, and the wound healed by granulation in the course of a month. It was thought that the blood had escaped through a rent in the external saphena vein, near its junction with the popliteal; but whether such a lesion was discovered is not stated.

Emmert,[3] of Berne, in an essay entitled "On Blood-Tumors of the Extremities, Caused by Laceration of the Veins," gives the following case: "A peasant woman, forty-five years old, fell upon her left knee, which soon became considerably swollen, red, and very painful. Spirit lotions were used without effect; the swelling increased, and showed such signs of inflammation, that a physician who was called, suspecting the presence of matter under the skin, made an incision into the tumor on its outer side. Instead of matter, blood, partly fluid and partly coagulated, escaped from the wound. The physician, fearing that the case was one of aneurism, closed the wound, and summoned Emmert in consultation, who found a large swelling of the knee and lower part of the thigh, most marked on the outer side. Just below the joint the tumor was distinctly defined, while above its outline was gradually lost in the œdematous tissues of the thigh. On the anterior part of the knee fluctuation was evident; the patella could be felt by deep palpation, and was in its normal position; the tumor was tense, the skin covering it being stretched, highly inflamed, and, in some places, already mortified; the incision was filled with a blood clot, and pulsation could not be discovered. Knee and hip-joints rigid, pain severe, leg normal, general condition feverish. The nature of the swelling seemed doubtful, but as it was imagined to be an extravasation, due to the rupture of a small aneurismal sac, a provisional ligature was placed around the femoral artery before the tumor was laid open, in order to guard against severe hemorrhage, in case this should occur. An incision between seven and eight inches in length was made on the outer side of the swelling, whereupon its entire contents, consisting of two to three pounds of coagulated blood, escaped, or were removed, from the wound. No trace of pus was found, nor did any fresh bleeding take place. After the cavity had been cleaned by the injection of cold water, it was ascertained that the extravasated blood had accumulated between the skin and the muscles. The former was much attenuated, especially at the points where gangrene had occurred. No proper sac could be discovered. The empty space beneath the skin was extensive, but the source of the bleeding could not be detected, either by the sight or the touch. The femoral and popliteal arteries pulsated strongly, and along the course of the femoral vein could be felt marked varicose swellings. After removing the gangrenous integument, a dressing was applied. The following morning it was discovered that a considerable hemorrhage had taken place in consequence of oozing; this was arrested by the use of cold compresses. A few weeks later the patient was discharged cured."

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[1] "Of Tumors Formed by Ruptured Veins Sometimes Mistaken for Aneurysms." By Mr. Else, Surgeon to St. Thomas' Hospital. "Medical Observations and Inquiries," vol. iii., p. 169, London, 1769.

[2] "Essay on the Pathology of the Blood," Edinburgh, 1858, p. 366.

[3] *Schmidt's Jahrbücher*, 1843, Bd. 38, S. 328.

Violent muscular contraction sometimes causes the lesion under consideration. Hodgson[1] describes two cases in which a vein in the calf of the leg was ruptured during a severe attack of cramp in the gastrocnemius muscle. A more important case is related by Else[2]: A man, aged twenty-five, was seized with a pain in his leg while lifting a heavy weight. A hæmatoma followed and was opened, discharging a large amount of blood. The case was mistaken for one of aneurism, and the limb was amputated. On dissecting it, the hemorrhage was found to have proceeded from a large vein which was ruptured. The edges of the rupture, which was situated just above a pair of valves, were torn and irregular.

Most surgeons have doubtless met with examples of deep-seated hæmatoma of the leg, occurring in persons whose veins are varicose, and in which the extravasation may be fairly attributed to the accidental rupture of some one of these dilated vessels. Sometimes the accident occurs without any sudden or violent muscular effort. Verneuil,[3] in 1855, refuted the opinion then prevalent, that only the superficial veins were liable to become varicose, and demonstrated the frequent existence of deep-seated varices, both intermuscular and intramuscular, in the lower extremities. In many cases the lateral dilatations of the walls of the veins were so weak that they burst when even a careful attempt was made to inject them. The same writer,[1] in 1877, showed that certain cases of that vague affection termed "coup de fouet" were characterized by deep-seated hemorrhagic extravasations, apparently dependent on the rupture of varicose veins, inasmuch as the subjects attacked exhibited well-marked varicosities of the subcutaneous veins. Several such cases are related in which, from thrombosis or embolism, serious or fatal consequences resulted.

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[1] "A Treatise on the Diseases of Arteries and Veins," London, 1815, p. 520.

[2] *Op. cit.*, p. 174.

[3] Du siége réel et primitif des varices du membre inférieur.—*Gazette médicale de Paris*, 1855, p. 534.

Finally, Legouest[2] has recorded four cases of hæmatoma believed by him to be due to the spontaneous rupture of a vein. The first one is quoted from Monlinié; the others were observed by himself.

CASE 1.—A man, aged fifty-nine, was admitted into the hospital on account of an enormous swelling of the right leg, the skin of which was tense, shining, and of a dark red color. The disease had been regarded as erysipelas, with a tendency to gangrene; but the signs of inflammation were wanting. Obscure, deep fluctuation suggested the possibility of an abscess, but only blood escaped when the swelling was incised. Various local remedies were then used without benefit; the tumefaction increased; the skin became thinner; blood, mixed with serum, continued to flow from the incision, and the connective tissue softened and broke down. Pus was not secreted, but exuberant granulations formed, which were destroyed by the actual cautery. Under firm compression, the limb gradually regained its natural size and shape.

CASE 2.—This occurred in 1857, in a military hospital in Algiers. A robust fisherman, forty-five years old, was seized with a severe pain in the left leg, shortly after lifting some heavy packages.

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[1] De certaines formes graves du coup de fouet.—*Archives générales de médecine*, 1877, tome i, p. 24.

[2] De la rupture spontanée des veines.—*Archives générales de médecine*, 1867, tome i, p. 513.

The limb was swollen twice its natural size from the knee to the ankle, and was cold, dark-colored, and numb. Voluntary movements were abolished in the affected parts. The patient's general condition resembled that of shock from injury, being marked by great prostration, slow and thready pulse, cold and clammy skin, and involuntary fæcal evacuations. The mind, although sluggish, was clear. Death by exhaustion occurred on the night following admission. Post-mortem examination failed to discover any thing abnormal except in the affected leg, the subcutaneous connective tissue of which, as well as that between the superficial layer of muscles, was uniformly infiltrated with blood. The color of the muscles was darker than natural. There was no evidence of gangrene, nor any circumscribed collection of blood. The deeper arteries and veins were intact and healthy; the superficial veins were lost in the extravasation and could not be traced.

CASE 3.—A physician, aged forty-nine, during the Italian campaign in 1859, suddenly felt one morning a slight pain in the joint between the second and third phalanges of the right hand, soon followed by a sensation of numbness in the entire finger, which became swollen, tense, and ecchymotic. The patient believed that the trouble had been caused by some violent movement he had made with his hand a few hours previously, while riding an unruly horse. Eight days later he was aroused from sleep by a severe pain in the distal articulation of the same finger, which soon became more deeply discolored by a fresh extravasation, signs of which extended to the back of the hand, which was swollen and stiff. All traces of the disease disappeared a fortnight after its commencement.

CASE 4.—A strong, well-built soldier, aged twenty-two, suddenly experienced a numbness affecting the whole of his right hand, which he remembered to have sprained five months previously. Intense pain followed almost immediately, and, four hours later, the hand was black, and greatly swollen, and had lost sensibility and motion. The symptoms were evidently owing to an extravasation of blood, and subsided in the course of two weeks, leaving the patient entirely well.

I have cited these cases reported by Legouest, because they are offered as examples of spontaneous rupture of veins; but no such lesion was demonstrated in any one of them, and its existence in all except the second is at least doubtful. Indeed, in many cases of extensive extravasation, whether due to traumatic or other causes, it is evidently impossible to reach a positive conclusion regarding the source of the hemorrhage, which may have proceeded from arteries, veins, or capillaries. When the extravasated blood is diffused, as is observed in ecchymosis, it disappears almost always by undergoing absorption ; and with rare exceptions, this occurs likewise when the collection is circumscribed. In practice, therefore, the rule is to adopt such simple measures as are calculated to promote this result, avoiding operative interference, unless suppuration takes place, or distension becomes so great as to threaten the occurrence of gangrene. A recent experience, however, has taught me that a ruptured vein may behave like a ruptured artery, causing an affection analogous to that named traumatic false aneurism, and requiring similar treatment for its relief.

On June 10, 1883, I saw at Port Richmond, in consultation with Drs. Walser and Blondel, a gentleman fifty-one years of age, concerning whom I obtained the following history :

On April 9, 1883, while walking in the street, he was suddenly seized with a sharp pain in the left thigh, so severe as to compel him to return home in a carriage. He was visited a few hours later by Dr. Eugene Blondel, and had been confined to his bed ever since. An examination of the limb at that time failed to reveal the cause of the pain, to allay which morphia was administered. On the day following the attack, slight swelling of the thigh was noticed, and, three or four days afterward, marked ecchymosis of the scrotum and upper part of the thigh on its anterior aspect. The further progress of the case up to the time when it came under my observation was marked by continued pain, increasing swelling, and extensive discoloration, involving the greater part of the thigh on the inner side and posteriorly. No positive diagnosis had been made. Toward the end of April he was seen by a consulting physician, who imagined the disease to be scorbutic, and recommended the use of iron. Subsequently, when the tumor had grown larger, it was suspected to be a traumatic aneurism, due to a rupture of the femoral artery. At the time of my first visit I made a careful examination of the swelling, which occupied the inner and posterior regions of the upper two thirds of the thigh. The circumference of the affected limb was six inches greater than that of its fellow. The skin covering the tumor was stained in various hues by blood which had evidently come from a deep-seated extravasation. Much of the swelling was indurated, but fluctuation was well marked at its lower and inner part. At first sight it seemed to pulsate. An attentive examination, however, showed that the pulsation was limited to the femoral artery. This vessel could be seen and felt beating from Scarpa's space as far down as the lower end of Hunter's canal, which had apparently become superficial in consequence of a displacement of the sartorius muscle, due to the pressure exerted by the large swelling beneath it. The tension of the tumor was only moderate, but the patient alleged that at times it had been very great. I failed to appreciate the importance of this statement until it was recalled to me by a circumstance presently to be mentioned. Compression of the tumor was unaccompanied with any sensible diminution in its bulk, nor could any thrill or murmur be detected in any part of it. Heart and arteries apparently normal. Pulsation of both posterior tibials equally distinct. Right internal saphena vein varicose.

A careful consideration of the case led me to regard the swelling as a hæmatoma, caused by the spontaneous rupture of one of the deeper veins. As the integument seemed to be thinned at the spot where fluctuation was most distinct, and as the patient had recently had a chill, followed by considerable constitutional disturbance, I imagined that suppuration had taken place, and recommended an exploration of the swelling, with the object of liberating its contents by a free incision, in case this supposition should prove correct.

June 11th. Patient etherized. Punctured tumor in four places with a coarse aspirating needle attached to a hypodermic syringe. No pus was withdrawn, but only old, dark-colored, semi-fluid blood, which was not putrid. I now decided to avoid laying open the cavity, if possible, hoping that the sanguineous effusion might yet be removed by absorption.

June 28th. Since last date but little change occurred in the patient's condition until two days ago, when the temperature rose to 103°, and the tumor became softer. Yesterday a red, tender patch, about three inches in diameter, appeared on the right arm, and was regarded as a probable indication of septic poisoning. Again suppuration of the sac was suspected, and preparations were immediately made to empty it by a free incision. But, during the administration of ether, an extraordinary alteration was observed to take place in the tumor, which suddenly grew large, and became exceedingly firm and elastic. I could account for such a change only upon the supposition that the cavity of the tumor was in direct communication with some large blood-vessel. We therefore concluded to postpone operating until the following day, when we might come prepared to deal with what was evidently a formidable blood-tumor. Meanwhile, the patient gave his consent to amputation, in case it should be thought advisable to perform it.

June 29th. Operation. Present: Drs. Walser, Blondel, and Whitman. Digital pressure being made by Dr. Walser upon the femoral vessels in the groin, I incised the swelling, which was found to be filled with blood, mostly coagulated. At first the incision was made only large enough to admit two fingers, with which I broke down the clots, in order to facilitate their expulsion by external pressure. Most of the coagula were soft and dark-colored, but those which escaped last, and which formed a globular mass about the size of a lemon, were light in color, quite firm, and distinctly laminated. When the sac was emptied, the blood which had been collected was found to measure seven pints, and it was estimated that the quantity not measured amounted to one pint. The presence of an open vessel being indicated by a gush of fresh blood immediately following the removal of the last clots, an Esmarch's elastic tourniquet was applied to the upper part of the thigh, and the sac was slit up by extending the incision upward and downward until it measured about nine inches. This allowed a satisfactory inspection of the cavity, at the bottom of which, near the middle of the shaft of the femur, dark-colored blood was seen trickling from a lateral opening in a vein of considerable size, the circulation through which was not completely arrested by the elastic tourniquet. The direction and situation of the vein was that of the venæ comites of the profunda artery, and the opening into it readily admitted a large silver probe, which passed both upward and downward an inch without encountering resistance. Owing to the great depth at which the vessel was placed, and the plastic infiltration of the surrounding parts, I did not attempt to isolate it; but by transfixing, with a somewhat sharp-pointed aneurism-needle having a very wide curve, the tissues in which the vein was imbedded, I succeeded, after several trials, in tying it above and below the opening. The venous bleeding ceased immediately after the ligatures were applied, and did not recur when the tourniquet was removed. At this moment, however, slight arterial hemorrhage took place from a small vessel which lay in the track of the aneurism-needle, at some distance from the vein, and had doubtless been wounded by it. The vessel was readily seized with an artery forceps and secured by a ligature, whereupon all hemorrhage was arrested. Very little blood was lost by the operation, and it was interesting to observe that when the tourniquet was removed, no oozing occurred, the vast cavity being lined with a layer of fibrin, which contained few or no blood-vessels. No proper membrane existed to form the sac, which was simply limited by the connective tissue pushed aside and condensed by the extravasated blood, and lined in the manner just described. The operation was completed by stuffing the cavity with iodoform gauze, and draining it by means of a stout india-rubber tube passed through a counter-opening opposite the great trochanter. No sutures were employed.

The case was left in charge of Dr. Walser, who afterward wrote to me as follows: "There were only four antiseptic dressings applied. At the end of six weeks there was nothing left unhealed but a sinus, which eventually discharged a ligature, and then closed." When I next saw the patient, four months after the operation, he had quite regained his health and strength, and weighed one hundred and eighty pounds. A note from Dr. Blondel, dated October 29, 1884, informs me that he still remains quite well, and is entirely free from trouble in the affected limb.

In reviewing this singular case, it is first natural to inquire what may have been the cause of the lesion described. The absence of cardiac disease, external violence, or unusual muscular exertion, warrants the suspicion that the coats of the vein were abnormally weak at the seat of rupture. This view is corroborated by the fact that the long saphena vein of the opposite limb was varicose; for it seems not unreasonable to suppose that a similar condition may have existed in the deeper veins, thereby predisposing them to rupture, either spontaneously, or in consequence of the increased blood-pressure known to be present during ordinary exercise.

In the second place, the case I have related proves that the rupture of a vein may lead to consequences closely resembling those which result from the rupture of an artery when this is followed by what is often called false aneurism. The slight blood-pressure existing in the veins as compared with the arteries, and the tendency of the former to collapse, when divided or empty, are circumstances which usually suffice to determine the closure of an artificial opening and prevent indefinite extravasation. But if the opening is large, and the vein is surrounded by unyielding tissues, the hemorrhage may continue, and cause the formation of a cystic tumor, which communicates directly with the blood-vessel, and which differs from a false aneurism only in the fact that it communicates with a vein instead of an artery. The signs by which such a tumor can be distinguished from an aneurism need not be mentioned here, but it may be interesting to note two points of contrast between it and an ordinary hæmatoma. In the latter affection, in which no permanent communication is established between the blood-cavity and the circulating vessels, the extravasation will attain its maximum at an early period; and no further increase in the size of the swelling will occur, except in the event of suppuration, which can always be demonstrated by an exploratory puncture. If, however, the vessel from which the blood has escaped remains open, fresh hemorrhages will be likely to take place from time to time, causing a corresponding enlargement of the swelling.

The other point of contrast between such a tumor as I have described and an ordinary hæmatoma, is one which is liable to be overlooked, but which, when verified, is both striking and conclusive. This is the remarkable change that sometimes occurs in the swelling, which suddenly, in the course of a few minutes, may undergo a decided increase in bulk, and become extremely tense and elastic. Such an alteration can be due only to a fresh extravasation, and affords almost positive evidence of the existence of a free communication between an open blood-vessel and the sac. In a doubtful case, therefore, this symptom, which is perhaps pathognomonic, should be carefully watched for, as it occurs only at intervals, and is of brief duration, the extreme distension subsiding either because a part of the extravasation re-enters the vein, or because it undergoes absorption.

Finally, the analogy between a blood-cyst communicating with a ruptured vein and one connected with a ruptured artery extends to the principles of treatment. In the case of rupture of an artery, whether partial or complete, if nature fails to heal the lesion, it is a well-established rule to lay open the blood-cavity, and tie the artery above and below the bleeding point. The case I have recorded proves that, when the open vessel is a vein, a resort to the ligature may be equally successful; and perhaps future experience will show that it is equally imperative.

METALLOSCOPY AND METALLOTHERAPY.

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BY metallotherapy is meant the rational treatment by metals, externally applied or internally administered, of nervous and other disorders wherein sensibility and motility are markedly disturbed.

To Dr. V. Burq, of Paris, belongs the honor of originating this new therapy; hence the name Burquism under which it is now generally designated.

We think it well to mention at the outset one of the points most strenuously insisted on by Dr. Burq, namely: the therapeutic susceptibility of the individual, in illustration of which we will instance anæmia. In one this will be found amenable to iron, in another to copper, to gold, etc.; the curative agent varying with the individual's metallic susceptibility, whereby we prescribe; and not by label, as is too often done with syphilis and mercury, chlorosis and iron, skin diseases and arsenic.

Metalloscopy is the process whereby the metallic sensibility is determined. But prior to giving its rules it will be in place first to consider the doctrine of Burquism, which, in *verba magistri*, is as follows:

Neuroses may be divided into two great classes:

1. Those where in there is constantly a loss, total or partial, of general or special sensibility and muscular force.

2. Those wherein neither is affected, as in epilepsy.

The neuroses of the first class, especially hysteria, may show themselves under so many fleeting forms and varied aspects that to aim at describing them would be about as quixotic as to try to set down all the shrill and discordant sounds of a violin with three strings, whereof one would represent sensibility, another motility, and the third the intellectual status.

And yet however great their number and variety, all the symptoms of neuroses of sensibility and motility—I omit designedly those affecting the intelligence—may be classified under the three following groups, namely:

1. Group of hyponervous symptoms, which may be designated by the sign *minus* (—), under which we include all negative or *minus* disorders.

a. Of general sensibility: *analgesia and anæsthesia.*

b. Of special sensibility: *amblyopia*, *achromatopsia*, *anosmia*, *acousia*, etc.

c. Of the genital sense: *impotence*, *sterility*, etc.

d. Of motility: *amyosthenia*, *paresis*, *paralysis*, etc.

e. Of the vaso-motors: *athermia*, *ischæmia of the capillaries*, *dysmenorrhœa*, *amenorrhœa*, etc.

2. Group of hypernervous symptoms, to which may be given the sign *plus* (+), under which we include all positive or *plus* disorders of the above-mentioned functions.

f. *Neuralgias*, *visceralgias*, *cutaneous* and *sensorial hyperæsthesias* of every description.

g. Clonic and tonic spasms, which manifest themselves in the solid muscles by cramps, eclamptic or hysteric attacks, and contractures (club-foot, torticollis, strabismus, vaginismus, etc.); and in the hollow muscles by palpitations, coughs, (*toux férine*), asthma, incoercible vomiting, etc.

h. Elevation of temperature, even to the extent of simulating fevers of different types.

i. Various abnormal hypersecretions: *lachrymation*, *ptyalism*, *polyuria*, *hyperidrosis*, *leucorrhœa*, etc.

3. Group of ponderating symptoms—by and by we shall see why this sign is expressed by that of *equality* (=).

j. Various gastric disorders: *anorexia*, *dyspepsia*, and *gastralgia*, with their train of secondary symptoms, such as impoverishment of the blood in all its constituents, loss of flesh, flaccidity of tissue, puffiness, fluor albus, etc.; in a word, the cachexia inherent to the condition which constitutes chlorosis, chloro-anæmia, *pâles couleurs*, etc. of systematic writers.

The distinguishing traits of these three groups are as follows:

Those of group No. 1 (—) are permanent; they are the silent attendants of the neurosis in all its phases; they progress or retrograde, *pari passu*, with it; they are always the first to disappear or return again; absent only as long as the cure persists, their return unfailingly heralds an impending relapse; they keep, so to speak, step together; in fact, they are the neurosis' constant index, its pulse, and it is in this direction that it behooves the physician to be constantly on the alert.

On this account, says Dr. Burq, I never go without two instruments, the dynamometer and æsthesiometer.

The symptoms of the + group are as noisy as those of its opposite (—) are silent, for which reason they have ever enjoyed the baneful privilege of engrossing the attention of the profession as well as that of the laity. They are essentially transitory and constantly merge into one another, giving rise to the belief in a cure which does not exist.

When one of this group (+) remains unchanged for years (as in a case before us), it is exceptional; yet even then it coexists with other troubles which are intermittent. As I happen to mention this word, let me put you on your guard against it in neurology, and do not connect it with quinine as its antagonist. In this regard I could relate many a curious tale, how bottle upon bottle of the antiperiodic had been emptied in its name; about as fruitlessly and as often as has been done with iron in chlorosis, in conformity to the ancient theory of its reconstituent effect upon the red globule, an error against which I never omit entering a protest whenever opportunity occurs.

The disorders of group No. 3 (=) progress on parallel lines with those of the two other groups, and like group No. 1 are permanent. I call them ponderating because their action is to measure out the forces needed in striking them at the root—namely, in the stomach, which, as has been very truly said, "holds the keys to the house" (*tient les clefs de la maison*). For if this organ ceases to properly perform its functions there results an alteration in the production of nerve force.

All the neuroses of the first class (with anæsthesia and amyosthenia), whatever their apparent seat or the name they go by, constitute but one and the same affection, and require the same treatment.

For in all these neuroses it is constantly the same immediate, efficient cause—namely, a diminution in the normal expenditure of nerve force resulting from functional inadequacy of the skin and muscles, these two great waste producers of the economy. In every instance the result is necessarily the same: on the one side a proportionate artificial expenditure, most frequently intermittent, of the non-utilized force at any point of the organism, or maybe at several points simultaneously (+ or hypernervous symptoms); and on the other, gastric disturbances, to cut off the production supply. All such cases require alike for their cure previous and complete removal of all hyponervous disorders.

Proportionately as sensibility and motility become again normal, and thereby equalize physiological gain and waste, spasm in its turn (which has been wrongly defined "a contraction without purpose or reason"), neuralgia or delirium, which act as safety valves, gradually diminish; dyspepsia or gastralgia, its cause removed, stops in its work of dismantling the organism, and presently the patient finds in his food and a more perfect digestion all the elements required by the organism, viz.: iron, as well as albumen, fibrin, etc.

Hence this double conclusion:

"Anæsthesia and amyosthenia and all such disturbances are the proximate cause of all the disorders, and constitute, metaphorically speaking, a touchstone of the evil to surely indicate what is its best remedy.

"Given a nervous affection, with disturbed sensibility and motility, the entire problem consists in finding an agent, whatever it be, which will restore them both to a normal condition."

BASES OF BURQUISM.

Passing on to the question of practice, I will say, certain metals, those which nature supplies in most abundance, but in the native state, or one as near as possible thereto, iron first, next copper, then zinc, gold far ahead of silver, tin, platinum, aluminium, and some more, arsenic, mercury, manganese, and may be antimony, nickel, and many besides, which metallotherapy has not as yet been able to utilize, in virtue of a special action on the nervous system, be it dynamic, electric, or any thing else, it matters little, but which probably is exerted under conditions of particular affinity between these metals and different organisms, have the following peculiar property, namely: When applied to an anæsthetic surface, or over amyosthenic muscles, they generally produce a return of sensibility and muscular force in the parts covered by the metal first and next in their vicinity, and also increase the temperature and capillary circulation, at the same time producing subjective phenomena of heat, formication, etc., in the member under experiment ; then, after a lapse of time proportionate to the extent of surface covered, as well as to the subject's nervosity, phenomena of an inverse order occur, *i. e.*, anæsthesia, and amyosthenia, now called post-metallic, accompanied by a sense of fatigue in the limb under experiment, but which may extend to the brain itself.

Any metal acting in the aforesaid manner, is the one characteristic of the patient's metallic or therapeutic sensibility ; and this metal, and it alone, applied not to the seat of disorder, but to the anæsthetic or amyosthenic parts, or internally administered in proper form, will soon produce in a permanent manner what it did but transitorily at first in the metalloscopic examination, and ultimately will cure, or, at the very least, notably improve the neurosis, acting at every point in a manner to confirm the doctrine I have just laid before you. To condense the entire question we will put it thus : Given a nervous affection with anæsthesia and amyosthenia, find a metal which will restore one or both. Such is the problem to be solved, and the solution will indicate the means of cure.

To illustrate, let us suppose a series of three neurotics with anæsthesia and amyosthenia. In No. 1 we find sensibility and muscular force increased by external application of iron ; in No. 2, by copper ; and in No. 3, by zinc. To the first we would administer iron, to the second copper, and zinc to the third, feeling sure of a favorable result in each case, *i. e.*, restoration of all functions in abeyance to a normal condition.

METALLOSCOPY.—For this examination, which may be performed by cutaneous application of discs or subcutaneous injection of very weak solutions of the oxides, four instruments are required, viz. :

Æsthesiometer,
Dynamometer,
Thermometer,
Sphygmograph.

We will not speak of the last instrument, as it is an unusual one and somewhat difficult of manipulation. Besides it could do no more than confirm the information given by the thermometer, dynamometer, and æsthesiometer.

The thermometer is of the spiroid surface kind, surrounded by an ivory wall, and covered in so as to make it almost a closed cavity. It is held in position by an elastic band with buckle. Between the mercury and ivory is placed a ring of the metal we wish to try. It is well to have two thermometers, one for each side, and apply them simultaneously.

The dynamometer is a hand one, with self-registering index. It is used to ascertain the grip or strength of the muscles of each forearm, which may be assumed to represent that of the entire muscular system under ordinary circumstances.

The æsthesiometer resembles a compass with blunt points, and has a cross-bar to indicate the distance between its points. At its extremities are two concealed needles which may be used to test sensibility to pain. They are made to protrude in the same way as the lead in a metallic pencil, by screw action.

In order to clearly indicate the procedure at every step, we will suppose we have to deal with a typical case, presenting the usual symptoms of hemianæsthenia and amyosthenia, which we always look for in the upper extremities, as this is their seat of predeliction. Our first step would be to ascertain the extent of the former, accurately with the æsthesiometer, or more roughly by transfixing the skin with a needle, noting at the same time if blood follows the pricks. The second we determine with the dynamometer. We next proceed to take the temperature of each forearm; and when found, say in 10–20 min., we slip under the thermometer on the affected side the metal we wish to try. Now, if it produce, after from 1–15 minutes, certain phenomena, such as increase of temperature and muscular force, abolition of anæsthesia, bleeding of the formerly exsanguined needle-pricks, with heat, tingling, burning, numbness, and heaviness of limb experimented on, together with local redness and sweating of parts covered by the metal, we may safely conclude we are in possession of the active metal, and proceed at once to give it internally.

Such are the phenomena produced by the metal suited to the patient's idiosyncrasy; all others, except in a few rare instances, produce no effect whatever.

If we keep on with the application of the metal in the above instance, after a lapse of time of variable duration, another set of symptoms occur of an inverse order, called post-metallic. Anæsthesia is the first to return, then amyosthenia; the needle-prods cease to bleed, and the temperature falls.

If, however, we stop at the point where sensibility is at its highest, as well as muscular strength and temperature, the improvement remains and very often extends to the whole body, to gradually disappear in a day or two.

We will merely mention that in some cases we get a transfer—that is, the hemianæsthetic side gains by application of appropriate metal exactly and symmetrically what the other side loses.

Taking, for instance, a right hemianæsthesia susceptible to gold: If we apply three or four five-dollar gold pieces to the right forearm, we shall find, as sensibility returns, that it is disappearing at identical points on the opposite side, until ultimately the right side becomes of normal sensibility, and the left anæsthetic.

This transfer holds good for special senses too, as, for example, in acousia and dyschromatopsia the ear or eye affected gains evenly what is lost in the other.

Metalloscopy also furnishes us with the means of knowing if our patient is cured. Thus, supposing the treatment to have been carried out for four or six weeks and all the functions appear to be again in a normal condition and we want to be sure of it. For this purpose we apply several discs of the metal we had found in our first metalloscopic examination, to the formerly anæsthetic side. If the cure is complete, the result will be negative; if only apparent, there will be a return of all the old symptoms in inverse order.

Occasionally we meet with what is termed a hidden or latent metallic aptitude—that is, anæsthesia and amyosthenia exist, but are uninfluenced by any metal externally applied,—and this sometimes happens in those whose metallic aptitude is already known. In this latter case, all we need do is to give internally the active metal; in a few days its external application will again bring about the usual results. If, however, we are ignorant of our patient's idiosyncrasy, it will tax our ingenuity to discover it; unless we find by inquiry there is a susceptibility to mesmerism or somnambulism which always indicate copper. We may also gain some hints by inquiring what metals patient has ever taken, and if amongst them there has been one which increased appetite, etc., we may administer it and try external application in a few days to confirm our suspicion.

There are a few practical points it may be well to mention for those who have not the necessary instruments or time to go through a metalloscopic examination *en règle.* Taking cutaneous sensibility for our guide: For iron, all we need is a needle and a piece of steel, such as a steel thimble, a blade of knife, etc.; for copper, a piece of brass, such as bell brass; for gold and silver, pieces of coin, and so on with all the metals, excepting aluminium and platinum, which can only be had of the instrument-maker.

An examination of this kind requires only from one to five minutes, and the needle-pricks will indicate with sufficient accuracy the cutaneous sensibility.

Metals in a pure state are generally inactive and not unfrequently altogether inert. Experience has shown the following combinations to be the most active:

1. Iron combined with carbon, or steel.
2. Copper, alloyed to 8 to 10 % with zinc.
3. Gold, coin standard.
4. Zinc, alloyed to 5 to 6 % with tin.
5. Silver, coin standard.
6. Tin, alloyed to 3 to 4 % with regulus.
7. Platinum, pure.
8. Aluminium, pure.

The above is also the order of their utility, according to Dr. Burq.

TWO CASES OF HEMORRHAGE INTO THE SPINAL CANAL, ONE ACCOMPANIED BY FRACTURE OF THE PELVIS, THE OTHER BY IMPACTION OF THE BODIES OF THE DORSO-LUMBAR VERTEBRÆ; BOTH TENDING TO RECOVERY.

BY J. S. WIGHT, M.D.,

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CASE 1.—L. W., a seaman, twenty-six years of age, fell from the rigging of a ship to the deck, on the 14th of February, 1884, striking on his left side, and was taken care of by the captain till the ship arrived in port, on the 22d of February, when he was admitted to the Long Island College Hospital, where I examined him the day of his admission, and found the following signs and symptoms: The patient was lying quite helpless in bed. His face wore an anxious expression. He had some slight contusions on the left side of his head, and appeared to have had some concussion of the brain, the effects of which had mainly passed away. There had been a very severe and extensive contusion of the soft parts from the crest of the left ilium to the trochanteric region, as shown by marked extravasation of blood; and the signs of this extravasation continued for several weeks. I made a thorough examination as to fracture of the thigh-bones; neither the shafts nor the necks of these bones were broken. There was no apparent deformity about the pelvis. I put one hand on the right ilium, and the other hand on the left, and made considerable pressure, when the patient cried out with pain; and as I repeated the pressure, I thought I could detect mobility of the parts. So far as I could form an opinion, there had been a fracture of the pelvis through the horizontal and descending rami of the pubes, and there had been some separation of the left sacro-iliac synchondrosis.

The motion and sensation of the lower limbs had been disordered. The voluntary motion of the left lower limb had been quite completely abolished, and the left great toe dropped down just as if there had been total loss of power in the extensor pollicis, and some contracture of the flexor pollicis. The patient had some power of voluntary motion in the right lower limb. He could draw it up and rotate it to some extent, yet the disability was considerable. The sensation in the right lower limb was quite abolished. The prick of a pin below the knee made no impression. No reflex in the right lower limb could be detected. The sensation in the left lower limb was exaggerated. The slightest touch not only caused pain, but produced painful contractions of the muscles which had been deprived of voluntary control. The small vessels on the left side were dilated, especially in the foot and the lower part of the leg, and this condition led subsequently to considerable swelling. The temperature, as tested by surface thermometers, on the right ankle was 84 degrees, on left ankle was 88 degrees. There was not only vaso-motor paralysis, but there was augmented oxygenization in the fluids and tissues of the affected parts on the left side.

And now we must make our diagnosis include more than has been already expressed : either the sacrum must have been fractured or there must have been some hemorrhage into its central cavity, in order to produce the phenomena we have just described, for I suppose we may dismiss concussion of the spine as an adequate cause in this case. The central portions of the motor nerves on the left side have lost their power, while the central parts of the sensory nerves on the same side are in a state of irritation. On the other hand, the central parts of the sensory nerves on the right side have lost their power, while the central parts of motor nerves of the same side are perhaps somewhat irritated. The vaso-motor nerves on the left side have lost their power, while on the right side they are so far irritated as to produce some spasm of the small blood-vessels.

At the time of examination of this case—and with a section of the medical class present—I argued in the following way : There is in this case a fracture of the pelvis, and perhaps a fracture of the sacrum, and doubtless there is hemorrhage in the sacral canal. But in order to interpret the phenomena of disordered sensation and voluntary motion, as well as involuntary motion, we may, with much reason, suppose that, as the patient came down, he struck on his pelvis first, having his spine bent in the dorso-lumbar region, and that this part of the spine was wrenched and sprained so as to lacerate the blood-vessels at that point, and that there was hemorrhage into the spinal canal, on the left side of the lower extremity of the spinal cord, as it were—an apoplexy, but, to speak more correctly, a compression of the spinal cord due to hemorrhage. Now this blood-clot interferes with the power of the cells on the left side of the cord—and the motion of the left lower limb is abolished, and the sensation of the right lower limb is also abolished, for the nerve-cells and the nerve-fibres of voluntary motion are on the same side. And this same blood-clot interferes indirectly with the power of the cells of the right side of the cord, and this interference is in the form of an irritation, that is most decided among the cells of sensation on the right side of the cord, the effect being most manifest in the hyperæsthesia of the left lower limb. And in the next place there is a mild irritation in the cord that eventuates in contraction of the small blood-vessels of the right lower limb, so that less blood flows in the ankle and foot, and so that the local temperature is diminished. And finally, there is compression in the cord that eventuates in dilatation of the small blood-vessels of the left lower limb, so that more blood flows in the ankle and foot, and so that the temperature is augmented.

And now, if all this I have said with some detail is correct, this patient has some chances of improvement as time goes on; and if he recovers, may we not infer that our estimate of his case is substantially true? And to aid in his recovery we must adopt a judicious plan of treatment. In the first place he is helpless, and he must not only have a good bed, but he must be carefully turned from one side to the other, to prevent too long pressure on dependent parts. His ankles must be supported by cotton-wool, to keep the heels from too long contact with the bed. We will give him every morning a teaspoonful of Rochelle salts in a glass of water; this will have a beneficial effect on both the bowels and the kidneys. He may take twenty drops of the muriated tincture of iron three times a day in water. Let him have a plenty of milk, and a moderate quantity of solid food. Keep him on a wire bed, and see to it that he is always clean and comfortable.

This patient was despondent and complaining from time to time, having fears that he would not recover. About the 1st of May he had so far improved as to be able to sit up some; and after a few days—about the middle of May—his left foot was considerably swollen. A bandage improved the swelling. The last week of the month the toe-drop began to get better. All this time the left foot was painful. My patient watched all night with another patient the 2d of June. He had been using crutches for several days. His improvement continued, and he was able to be in the open air much of the time. On June 22d he tried to walk without his crutches, but his thigh pained him so much that he failed. On June 26th he was so much better that he was sent home to England by steamer. He has further improved since he has been at home.

Case 2.—J. D., a carpenter, twenty-six years of age, April 17, 1884, fell from a sky-light a distance of fifteen feet, and, standing upright, struck on the bottoms of both feet, and then *went into a heap*, when he was unable to get up. He was conveyed to the Long Island College Hospital by the ambulance on the same day, where, on making an examination, I found the following signs and symptoms: There was a projection of moderate size in the dorso-lumbar region, as if the bodies of the last dorsal and first lumbar vertebræ occupied less vertical space than normal. After careful examination of the lower limbs, I found no fracture or dislocation. The patient could move his lower limbs to some extent.

He had more power over his thighs than his legs, and more power over his legs than his feet. So much for voluntary motion. There was some paralysis of the small blood-vessels of the legs and feet that caused moderate swelling, and this effect appeared to be about the same on both sides. In some respects there was total loss of sensation in the following regions: On the outside and bottom of the feet, as well as on the heels and some parts of the backs of the feet; on the outer and posterior aspects of the legs; on the posterior surfaces of the thighs; on the entire gluteal regions of both sides; on the penis, scrotum, and perineum; and on the recto-coccygeal surface. In any part of this territory a pin could be thrust to any depth without the least sign of pain. I repeated this experiment over and over again in the presence of the medical class, and always with the same result—no sensation. The territory in which sensation was lost appeared to be very nearly the territory of distribution of the cutaneous portion of the *sacral plexus*. But as soon as the pin-punctures came near the territory of the distribution of the cutaneous portion of the lumbar plexus, the patient began to cry out with pain, and when the pin-punctures got over into this territory, they seemed to show that there was an exaggerated sense of pain, as if the cutaneous ends of the nerves of the lumbar plexus were more excitable than normal. The rectum had not only lost its power to feel, but it had lost its power to move; the reflex function of the rectum had been abolished. The bladder had lost its contractile power, eventuating in retention and overflow of urine. This patient's injuries had been accompanied by very severe shock.

In the presence of the medical class I endeavored to interpret these phenomena, so interesting and so instructive, and I reasoned in the following manner: This patient came down on his feet. The superincumbent weight of his body bent the dorso-lumbar spine. The bodies of the last dorsal and the first lumbar vertebræ occupied less vertical space than before the fall. The cancellous bone was compacted —crushed. The ligaments of the posterior part of the spine were strained. The blood-vessels were pulled upon and some of them were lacerated, and so there was hemorrhage behind the spinal cord. The blood pressed on the posterior surface of that part of the cord that gives origin to the sacral plexus, and hence there is total obliteration of sensation in the territory of its distribution. And there is impairment of motion in that territory, because the anterior roots have not received so much damage as the posterior roots. And then it appears, that the cells in the vicinity, where the lumbar plexus begins, are in a state of irritation. They exaggerate the impressions made on the ends of their nerve-fibres. The conclusion then is, that there is a fracture of the dorso-lumbar spine—accompanied by hemorrhage into the spinal canal. I have seen several fractures of the spine caused by forcible bending of the body forward, and they have generally resulted in death. A case of this kind came under my care some years ago: A man in a small boat was run over by a tug. The man jumped into the water, and the tug struck him on the back of the head and the neck and came against the shoulders, and doubled him up, and broke his dorso-lumbar spine, and after a few weeks he died, when the bodies of two vertebræ were found compacted—say, crushed. Now this patient, that died, had a greater injury than the one that is at present before us. In a word, the prognosis is—uncertain. We cannot always be sure of the result in an injury to the spine. Yet sometimes we say that death will be inevitable. In the present case I am led to believe that there may be some improvement as time goes on; yet we must not be too confident and make promises that cannot be fulfilled. We doctors do not know every thing. If we keep our eyes open and our wits about us we may climb to a higher level in the future.

I may now add the following particulars of the clinical history of this case:

The water had to be drawn by catheter once or twice a day till the 27th of April, when the patient could pass his water without aid, though he could not then completely empty his bladder. About this time he could move his lower limbs better than he could when he was first injured. On the 30th of April the end of his penis began to become gangrenous. On the 1st of May there was incontinence of urine, as if the bladder had filled up and was running over. On the 2d of May bed-sores were observed on the right gluteal region and on the right trochanteric region, having a tendency to spread. About the 8th of May the condition of the penis was improving. And soon after the legs were stronger. On the 20th of May some sensation had returned to both legs. During all this time it was necessary to evacuate the rectum with an enema—which was accomplished by a long, flexible tube, introduced with great care. May 24th this patient was discharged from the hospital improved. His friends took him away—and I have not been able to find out how the case has been progressing since that time.

A CASE OF TRAUMATIC APHASIA.*

B. BRIBACH, M.D.,

ST. LOUIS, MO.

THE following case presents the interesting features of aphasia produced by pressure from the fractured skull, with entire absence of paralysis; of its complete relief by trephining, and of the fact that the seat of the injury did not exactly correspond with the location of the centre of speech.

Wilhelmina Müller, a healthy, stout country girl, eight years of age, had her skull fractured on the morning of June 28th, by the kick of a mule. She had walked some distance after recovering from a stupor of a few minutes' duration, and I saw her about an hour after the accident happened. She was in a badly frightened and excited condition, but entirely conscious, and walked freely about my office. The fracture was situated in the left temporal region, rather more than two inches long, one sixth of its extent involving the frontal, the rest the parietal bone. It had the shape of a curve, convex above, as represented in the diagram; the bone below and all along the line was markedly depressed. There was considerable difficulty in disengaging the flap of the cut scalp from between the fractured edges, into which it was firmly wedged, and I did so after giving chloroform. As there were no symptoms of either concussion or compression of the brain evident, I closed the wound by sutures.

After the child had recovered from the effects of the anæsthetic, it was found that she lacked the faculty of speech. I ascertained that she had not spoken any thing since receiving the injury. A close examination showed absence of paralysis of the muscles of the face, tongue, palate, and limbs. The facial expression was intelligent, and the girl showed evident anxiety to speak, finally making known by signs that she wanted water. Continued efforts to make her speak proved useless, and I directed the father to take her home and keep her quiet, and to induce her to communicate her wants by means of a slate and pencil.

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* Dr. Bribach published the substance of this case in the St. Louis *Courier of Medicine* for September, 1884, but the case seem so valuable as to deserve re-writing and illustrating. It may thus secure the full measure of notice it deserves at the hands of neurologists and surgeons.—EDITOR.

On June 29th, twenty-four hours after the accident had occurred, I found the child doing well, excepting the existence of aphasia. Pulse 90, full and moderately strong ; temperature normal ; appetite and secretions good. She had uttered only one word, "coffee," which she had made use of to indicate any of her wants. She had not made use of the slate, but whether from coexisting agraphia, or from lack of training to express her thoughts by means of writing, I was unable to ascertain. No symptoms of paralysis were discoverable anywhere. Patient efforts to make her speak were without result, and I inferred that I had to deal with a case of aphasia produced by pressure on the speech centre. The parents consented to the operation of trephining, which I deemed necessary to relieve the condition and to meet complications by subsequent inflammation. Dr. A. Montgomery, of St. Louis County, who kindly assisted me, concurred in my view of the case, and of the necessity of relieving the compression of the brain.

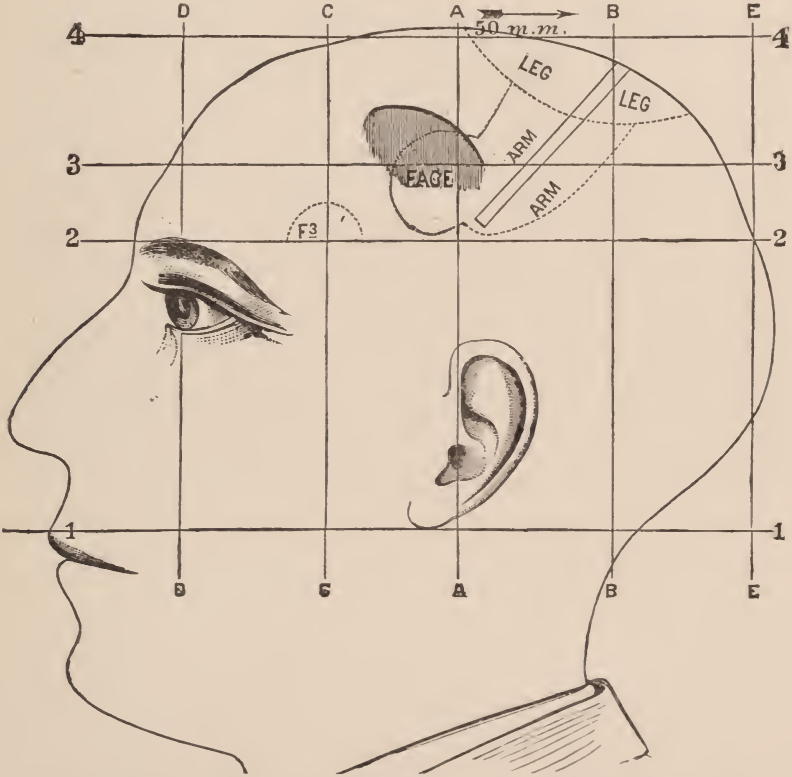
I trephined the same afternoon, thirty hours after the child was injured, and elevated the entire depressed portion of the skull. The dura mater was not injured, and the inner table of the skull was free from spicula.

June 30th.—Patient had passed a quiet night, and taken a hearty breakfast. Pulse and temperature normal. The mother informed me that the girl had slept over eight hours after the operation, and on awaking asked for a drink. In the morning she had again correctly named and asked for some playthings. During my presence the child could not be induced to speak.

July 1st.—I learned that patient had repeatedly spoken, but seemed to have been confused ; she would often call things by wrong names. On removing the dressing, she complained that I hurt her ; on this occasion I heard her speak for the first time.

The patient lived four miles from my place, I could see her but once a day, and had no opportunity to closely watch the manner in which the faculty of speech returned, For four or five days there was a certain amount of loss of verbal memory ; the speech was rather slow, the articulation perfect, but substitution of wrong names occurred frequently. At the end of ten days, about July 8th, she spoke about as well as before.

Surgically, the case progressed very favorably. There was no fever at any time ; I allowed the girl to get up at the end of the second week, the scalp-wound being then nearly closed. In the first and second weeks in August, two small splinters of bone were discharged from the wound over the seat of the trephined opening, and I subsequently removed one more, after which the wound



I. Lines of Cranio-Cerebral Topography Projected on a Profile of the Head.—1-1. Alveolo-condyloid plane (horizontal); 2-2, Fronto-lambdoid line ; 4-4, Vertex horizontal. A-A, Auriculo-bregmatic line passing through meatus auditorius, and drawn at right angles to line 1-1 ; at the top of the skull this line indicates the true bregma. B-B, vertical drawn parallel to A-A, 50 mm. (2 inches) posterior to it ; at its point of impact on the top of the skull this line crosses the posterior end of the fissure of Rolando, the so-called *Rolandic point*. The straight double line extending obliquely from the *Rolandic point* overlies the course of the fissure of Rolando, and is known as the *Rolandic line*. The areas marked off by dotted lines overlie the centres for the leg, arm, face, and for speech (motor function). THE HEAVY CONCAVE LINE WITH SHADING IMPINGING ON THE FACE CENTRE INDICATES THE SITE AND RELATIVE EXTENT OF THE CRANIAL FRACTURE AND DEPRESSION.

closed entirely. It is now covered by a thick cicatrix, the girl has attended school since the beginning of September, and is now as well as ever.

Comparing the seat of the injury with an adult skull, I concluded that the pressure of the fracture had been bearing on the third and ascending frontal and the ascending parietal convolution of the left hemisphere. Subsequent reference to Dr. Seguin's diagrams of cranio-cerebral topography, and exact measurements of the patient's head, showed that in this I was mistaken. The measurements of the child's head are eleven and a half inches from root of nose to occipital protuberance, twelve inches from right to left external meatus auditorius. A line drawn from the true bregma to a point midway between the external angle of the left orbit and the left meatus auditorious crosses the centre of the fracture three and one quarter inches distant from the bregma. Projections on Dr. Seguin's diagrams place the depressed area on the lower part of the ascending frontal, and but slightly on the posterior part of the third frontal, convolution, involving the face centre according to Dr. Seguin, the centre of articulation according to Ferrier. Assuming the relative locations of skull and brain to be identical with the patient and Dr. Seguin's diagrams, I can explain the involvement of the speech centre in this case only by assuming that the inferior depressed portion of the fracture exercised some indirect pressure on the speech centre, which is located anterior to the fracture.

ON A PECULIAR RINGED AFFECTION OF THE PREPUCE AND GLANS.*

By R. W. TAYLOR, M.D.,
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THE three cases which follow illustrate a peculiar and interesting affection which has not heretofore been described.

CASE I.—In January, 1874, an eminent practitioner of New York consulted me regarding an affection of the penis which caused him much uneasiness of mind. He was a large, portly man, thirty-eight years of age, having enjoyed perfect health, with the exception of occasional attacks of migraine. His family medical history was excellent, and he himself had led a prudent life. Some few weeks before he consulted me he felt slight pricking and even painful sensations in the penis toward the glans. As he walked, the movement of the organ or the pressure of the clothes caused slight uneasiness. Very soon he noticed certain peculiar scaling rings on the prepuce and glans, near the insertion of the frænum. When he came to me, the following appearances were noted: On retraction of the prepuce, chiefly on the left but also on the right side, there were in all about six scaling rings. These efflorescences were of a diameter of from one quarter to one third of an inch, and were very slightly reddened, covered with a thin, quite adherent layer of epidermis, and in width were about one third of one line. The redness, slight as it was, was sharply limited to the morbid circle, and the enclosed mucous membrane was apparently normal. If any of these rings were pinched between the fingers, no thickening was appreciable, nor did they in any way seem to cause any interference in the mobility of the parts. From a careful study I was convinced that the lesion consisted in hyperæmia and slight increase in the epithelial layers of the parts. The patient had consulted Drs. Van Buren and Weir, who had not at that time seen a similar case. His chief anxiety consisted in a fear that the affection was a commencing epithelioma. Though seemingly insignificant and superficial, these lesions were very persistent, and only disappeared after an existence of several months under an arsenical treatment. The patient took Fowler's solution in small doses, and it was only when fifteen drops thrice daily were taken that full involution of the rings occurred. As they disappeared, the itching, pricking, and painful sensations gradually subsided.

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* Read before the American Dermatological Association, Aug. 29, 1884.

Besides this peculiar eruption on the penis, a few months later the patient had three papules on the right foot, which appeared to be those of lichen planus. The number of papules was increased by the appearance of several on the dorsal aspect of the foot, but they did not disappear under the use of arsenic as did the rings on the penis. In September, 1875, these rings returned, and this time they looked shiny and micaceous. At the same time patient had the same painful sensations as in the former attack, and he felt spots on the penis, which presented no abnormal appearances, yet which were hyperæsthetic even upon light pressure. Besides these morbid phenomena, the patient suffered from an uneasy sensation about the right hip, upon the skin of which he had fugitive sensations, such as accompany goose-flesh. This attack lasted about two months, and left the patient well, except that he had the hyperæsthetic condition of penis upon movement or pressure. Early in January, 1876, the patient suffered from a relapse of his scaling affection of the penis, and at the same time he had several papules bearing the appearance of lichen planus. He underwent further treatment, was cured, and until to-day had not had a relapse, but the slight hyperæsthesia of penis is felt from time to time.

Case 2.—A gentleman, an American, of full habit, thirty years of age, having had gonorrhœa two years previously, which had been complicated by right orchi-epididymitis, consulted me in July, 1878, for an affection of the prepuce and glans. He was a man free from any diathesis, and of healthy parentage. His orchi-epididymitis, which had been treated by me, was followed by more or less severe paroxysmal attacks of hyperæsthesia and neuralgia of the right side of the scrotum and corresponding crural surface, with occasional fugitive neuralgic pains in the hip of the same side. Under internal and external remedies the induration in the epididymis was much resolved, and the painful sensations were nearly wholly relieved. Still any fatigue, exposure to cold, or excess in eating and drinking was almost certainly followed by an exacerbation of this trouble. Otherwise he had noticed nothing abnormal until May of 1878, two months previous to his visit to me in July of same year. He then began to experience a sensation of slight heat and itching in the prepuce and glans, which caused him to pull and press the end of the penis for relief. Occasionally this patient, as did the first, experienced a sensation as though a pin was pricking his prepuce. In the latter part of June, while on his return from the Continent, he found upon the prepuce, the fossa glandis, and on the posterior portion of the glans, several rings, which he said increased in number very rapidly until he came to me. I found about eight fully formed rings and a number of incomplete ones. On the right side of the prepuce a ring one half inch in diameter enclosed another of about half the size. These rings were similar to those of the first case, slightly elevated, though not perceptibly thickened. When examined by oblique light they were seen to be distinctly but slightly elevated. They were not very markedly hyperæmic, nor was there much hyperæmia at their margins. The scales covering them were of a dull gray color and quite adherent, but were thrown off in small quantities every day. This patient was also treated with arsenic, and at the end of a month, when he was taking ten drops of Fowler's solution thrice daily, these lesions disappeared. He had a similar relapse in 1879, and for more than a year experienced the peculiar neurotic sensations already spoken of.

CASE 3.—A gentleman, thirty-six years of age, thin but wiry, of nervous temperament, given to high living, consulted me January 27, 1884. He had enjoyed excellent health through life, but had suffered from gonorrhœa in 1874. In 1880 he had several chancroids, which had been very actively, even severely, cauterized by a country physician. Having a long, tight prepuce, he after this suffered from repeated attacks of balanitis. In July, 1883, he noticed a number of small rings on the glans and prepuce ; these were chiefly annoying by reason of the burning and pricking sensations which accompanied them. They disappeared under external treatment, and returned again in December of that year. When I saw him first in January, 1884, he had four typical rings such as were described in the first case. Under arsenic he was cured in four weeks. Locally he used simply a thin layer of absorbent cotton.

The clinical features of this interesting affection are, then, slightly elevated very narrow rings of reddened mucous membrane, with very well-marked but thin layer of epithelial scales superimposed. There was no areola; in fact the limitation of the morbid process was very sharp, the enclosed area of tissue being normal. In general, rings of from one quarter to one third of an inch were seen, but occasionally they had a diameter of half an inch. Segments of circles more or less complete were seen. The affection was preceded and accompanied in all instances by unpleasant painful sensations, which in one instance were very severe, and even persisted on the site of the lesion after their disappearance. No scar or loss of tissue was observed after their involution.

It would seem that the rings remain stationary for several weeks, and that these were liable to be followed by new ones, in all cases the prodromal nervous sensations being felt.

As to etiology, I am disposed to regard the cause as of neurotic origin. In the first case, though the patient suffered from migraine, there was no local cause discoverable; still the painful symptoms were very marked, and at one time were accompanied by painful sensations in the hip of the corresponding side. Of the two other cases, one patient suffered from orchi-epididymitis, and the other from balanitis and tenderness of the prepuce, due to severe cauterization. Instances are not infrequent, though, in which reflex pain about the scrotum and inguinal region are due to this affection of the testicle. Then, again, it is well known that relapsing herpes progenitalis has often followed severe cauterization in subjects previously free from that affection. Whether these analogical facts will help us in explaining the origin of the affection in the two last cases I am unable to say; still it is within the bounds of credence that the affection was in them due to reflex irritation. As to diagnosis, little need be said. The affection is so clearly marked in its appearance and history that it can be readily diagnosticated. It may be well to add that the ringed form of the erythematous syphilide, the same form of the papular syphilide, psoriasis in rings, tinea tonsurans (J. C. White), lupus erythematosus, may be found on the glans and prepuce.

The treatment found most efficacious by me was Fowler's solution of arsenic, pushed even to the extent of fifteen drops thrice daily.

EDITORIAL DEPARTMENT.

THE COAGULATION OF THE BLOOD.

By FREDERICK P. HENRY, M.D., Philadelphia.

The coagulation of the blood, although invariably treated of in physiological text-books, is in no sense of the term a physiological process, being a product of abnormal conditions. The blood circulating in the living, healthy vessels, never coagulates, but no sooner is the vessel-wall injured, however slightly, by a needle puncture or an escharotic, than a thrombus begins to form at the site of injury. This local coagulation may be regarded as a conservative effort to repair the lesion, but, if so, the remedy is frequently worse than the disease, for the thrombus may be swept away by the blood-current, and if finally arrested in a terminal artery—such, for example, as the arteria centralis retinæ—may work irreparable mischief. The coagulation of albuminous fluids, whether it take place within or without a cell-body, is a necrosis, the importance of which in pathological processes is just beginning to be recognized, and the time is probably not far distant when the recently introduced term, *coagulation necrosis*, will be considered tautological. Hayem speaks of the coagulation of the blood as a kind of rigor mortis—*une espèce de rigidité cadavérique*,—and this manner of regarding it is justified not only by the change in its physical properties, but also by the fact that coagulation never occurs without the destruction of some of the morphological elements of the blood. Nevertheless, the act of coagulation furnishes most important data for the study of the physiology of the blood, for it is a spontaneous analysis, the thoroughness of which depends upon the slowness with which it takes place. For this reason, horses' blood, which coagulates very slowly, is much employed in physiological researches. In such blood, before the clot has begun to form, the red corpuscles have gravitated to the bottom of the vessel, while the white remain at the top and form the buffy coat, and it has always been a matter of observation that, in such blood, the clot becomes gradually firmer as its upper layers are approached ; and, further, that a relation exists between the density and color of coagula. In the upper layers, where the color is lightest, the density is greatest, and *vice versa.* In other words, coagulation is most perfect where the white corpuscles are most abundant.

It would serve no practical purpose to give a history of the rise and progress of the various theories that have been upheld with regard to the coagulation of the blood, and their subsequent decline and fall ; but no paper on the subject can be regarded as complete, that does not contain some reference to the experiments made by Dr. Andrew Buchanan, of Glasgow, in 1831. This observer found that certain animal fluids, such as lymph—liquor pericardii—and non-inflammatory effusions, such as the fluids of hydrocele, hydrothorax, and ascites, which do not coagulate spontaneously, may be made to coagulate by the addition of the fluid obtained by squeezing a blood-clot in a linen cloth. He subsequently found, that the most effective method of causing the above-named liquids to coagulate was by adding to them what he called *washed blood-clot.* This he made by mixing blood with from six to ten parts of water and stirring for five minutes. The mixture was allowed to stand for from twelve to twenty-four hours, and then filtered through a coarse linen cloth ; the residue left upon the filter was then washed with water. This substance, mixed with a little spirit of wine to prevent putrefaction, retained its coagulative power for many months. This washed clot is a mixture of fibrin and white corpuscles, the red corpuscles having been dissolved in water and removed by filtration, and the question that presented itself to the mind of Buchanan was : Upon which of these ingredients does this coagulative power depend ?

He concluded that it was due to the white corpuscles, from the facts, that the upper layers of a clot, in which these bodies are most numerous, possessed a much greater coagulative power than portions taken from the lower layers, and that this coagulative property was unusually powerful in the buffy coat of horses' blood, which is almost entirely composed of white corpuscles. From these researches Buchanan concluded that fibrin exists in solution in serous fluids and in the liquor sanguinis, and that it has no spontaneous tendency to coagulate, but does so when brought in contact with a substance derived from the white corpuscles, which substance he considered to be of the nature of a ferment, for he compared its action to that of rennet upon milk.

These facts attracted little attention, and were almost entirely forgotten when Prof. A. Schmidt, of Dorpat, re-discovered them in 1861. He endeavored to isolate from blood the substance on which its coagulative power depends, and believed that he had identified it with serum-globulin, which he accordingly called the fibrinoplastic substance. He precipitated serum-globulin from serum by diluting it with water and passing through it a stream of CO_2, and demonstrated that serum from which the so-called fibrinoplastic substance had been thus precipitated had lost its coagulative power. Schmidt considers that there is little or no serum-globulin in the circulating blood, but that it is the product of the white corpuscles, which, according to him, undergo rapid destruction when the blood is withdrawn from the vessels. He found that plasma, from which the white corpuscles had been separated by filtration, was very poor in serum-globulin; while the white corpuscles remaining on the filter yielded a large percentage of this substance.

He next endeavored to ascertain upon what the coagulating property of transudates depends, and identified it with a substance belonging also to the class of globulins, to which he gave the name of fibrinogen. Transudates from which this substance had been precipitated no longer coagulated on the addition of the fibrinoplastic substance, and, on the other hand, when these two substances were isolated and mingled in alkaline solution containing a certain proportion of salts, the result was the formation of fibrin. Coagulation, Schmidt then supposed, was due to the interaction of these two bodies in saline solutions. He subsequently ascertained that the two bodies may be present in a fluid —that of hydrocele, for instance, which may contain considerable quantities of serum-globulin—without coagulating, but that a clot will form on the addition of blood or blood serum. It then appeared evident that a third factor was necessary to the act of coagulation, and this he named fibrin ferment. He obtained this substance from blood, or, preferably, serum, by mixing it with twenty times its volume of alcohol, and setting it aside for a considerable period of time—two weeks to three months. The alcohol coagulates all the proteids of the serum, which are collected on a filter, dried and pulverized, and the powder dissolved in water. Such a solution, when added to a mixture of fibrinogen and fibrinoplastin, that does not coagulate spontaneously, will often cause the prompt formation of a clot. Schmidt derives the ferment, as well as the serum-globulin (fibrinoplastin), from the white corpuscles; for he found that plasma, from which these bodies had been separated by filtration, when subjected to the process for obtaining ferment, yielded a very inactive solution of this substance, and that blood received directly from a vessel into absolute alcohol—by which means the destruction of the white corpuscles is supposed to be prevented—yielded no ferment whatever. He also demonstrated, by actual counts of the white corpuscles, that a large number of these bodies disappear in the act of coagulation, presumably partly in the formation of fibrin ferment. He counted the leucocytes in the plasma of horses' blood, and found that they numbered 14,909 per cubic millimetre in an average of eleven counts; he then allowed the plasma to coagulate and counted the leucocytes of the serum, and found that they numbered, in an average of nine counts, but 4,222 per cubic millimetre, 71.7 per cent. having disappeared in the coagulum. He also counted the red corpuscles before and after its defibrination, and found the difference to be only thirty per cent. He therefore argues that the disappearance of the leucocytes is not a mechanical entanglement in the meshes of the fibrin, for, were this the case, there would be no such difference in the percentage of disappearance between the red and white corpuscles. Besides, on examining a clot with the microscope, the red corpuscles may be seen entangled in the meshes of the fibrin, while of the white, not a trace remains but a few greatly altered cells, the majority having entirely disappeared in the formation of the clot.

Schmidt's theory of coagulation, therefore, is that it is due to the union of two substances, fibrinogen and serum-globulin, under the influence of a ferment, and that the fibrinogen exists as such in the circulating blood, while the serum-globulin and ferment are formed after the blood is withdrawn from the vessels.

The researches of Hammarsten tend to prove that serum-globulin is an unnecessary factor in the formation of fibrin, which, according to him, is derived solely from fibrinogen under the influence of the ferment. He has prepared fibrinogen free from all traces of serum-globulin, and has caused the formation of a clot by the addition of the ferment only. He admits that coagulation may be produced by the addition of serum-globulin to a transudation that will not coagulate in the presence of the ferment alone, but he has shown that other substances, such as calcium chloride and casein, have the same effect. He has also shown that, from fluids which do not coagulate on the addition of ferment, fibrinogen may be obtained which does coagulate when the ferment is added. In such a case the fluid evidently contained substances that prevent coagulation. Such substances are free alkalies, alkaline carbonates, and certain salts. For example, in a hydrocele fluid containing very little fibrinogen, that does not coagulate on the addition of the ferment and does coagulate on the further addition of calcium chloride, it is possible that this salt may decompose an alkaline carbonate that held the fibrin in solution. He claims also that when coagulation is produced by adding serum-globulin to a non-coagulating mixture of fibrinogen and ferment, the effect is due to impurities mingled with the serum-globulin, for when he repeated the same experiment with pure serum-globulin, coagulation did not take place.

Hammarsten, therefore, attributes the formation of fibrin to one substance, fibrinogen, which exists as such in the blood, under the influence of a ferment derived from the white blood corpuscles

The above-named observers agree in regarding fibrin as a precipitate caused by the union of soluble substances in the liquor sanguinis.

Later observations by Wooldridge, Norris, Hayem, and Bizzozero attribute the formation of fibrin, in great part, to a direct transformation of morphological elements of the blood.

In the Proceedings of the Royal Society for June 18, 1881, is an important contribution to the subject by Mr. L. C. Wooldridge. He obtained leucocytes from lymphatic glands, by a process described in his paper, and washed them thoroughly with a half-per-cent. solution of common salt, after which he found that their normal microscopic appearance was unaltered. He then found that if to one volume of suspended cells an equal volume of ten-per-cent. solution of common salt were added, the whole was immediately converted into a "peculiar semi-transparent jelly" which behaved chemically precisely like fibrin, while, under the microscope, he found that the cells, as such, had entirely disappeared. "Only nuclei imbedded in a distinctly fibrous ground-substance" were visible. Similar results were obtained on adding distilled water or a solution of magnesium sulphate. He then studied the behavior of leucocytes toward plasma. The most convenient way of obtaining plasma is by injecting peptone into the blood of an animal. If the animal be bled a few minutes after the injection, the blood does not coagulate, and by means of the centrifugal machine the corpuscles may be entirely separated from the plasma. Wooldridge obtained a specimen of this peptone plasma which presented the following characters :

It was totally uncoagulable : 1. On dilution with water. 2. On passing a current of CO_2 through it. 3. On addition of Schmidt's fibrin ferment. 4. On addition of serum-globulin. 5. On addition of normal serum. 6. On standing till it was foul.

Nevertheless, on adding leucocytes to such plasma, coagulation promptly followed, of which the completeness was in direct ratio to the number of leucocytes added. As additional facts in favor of his view that the plasma changes the leucocytes directly into fibrin, he gives:

1. The weight of the coagulum is, as near as may be, in such observations, identical with that of the cells that have been added.

2. The percentage of albumins in peptone plasma before coagulation is identical with the percentage after coagulation with cells.

3. The protoplasm of the cells has completely disappeared and has been converted into a partly fibrous, partly granular, ground-substance ; the nuclei remain.

4. If to a very large quantity of suspended cells (say 50 cubic centims.) a very small quantity (1 cubic centim.) of peptone plasma be added, the whole clots firmly.

Wooldridge does not deny that Hammarsten's theory of coagulation is partly true, for he concludes his paper with the statement that there are "two essential processes in the coagulation of the blood, one of which has been hitherto entirely wrongly appreciated or overlooked." This latter process is that the "dead" plasma converts the white corpuscles directly into fibrin. At the same time, however, that this occurs, a substance is liberated from the cells which converts the fibrinogen also into fibrin. This is the other process. The substance which is liberated from the cells is fibrin ferment.

Later observations by Norris, Hayem, and Bizzozero associate the formation of fibrin with changes in certain morphological elements of the blood: by the first, to changes in what he terms the invisible corpuscle or advanced lymph disc ; by the two latter, to similar changes in bodies termed by Hayem *hæmatoblasts*, from their supposed share in the formation of the red corpuscle, by Bizzozero, *Blutplättchen.* At the present time, the subject has assumed such a controversial aspect that it is somewhat difficult to arrive at an exact understanding of it. Thus Norris claims that the blood-plates[1] are nothing but products of disintegration of his colorless corpuscle, while Hayem and Bizzozero maintain that the corpuscle of Norris is nothing but a red corpuscle decolorized by manipulation. Certain it is, however, that the blood-plates may be seen in the circulating blood. These bodies (as shown by Osler) are the component constituents of Schultze's granule masses seen in blood by all microscopists. They are the elementary corpuscles of Zimmermann, the *globulins* of Donné, the *grains sarcodiques* of Vulpian, and the *granulations libres* of Ranvier. The latter regarded them as particles of fibrin which serve as centres of coagulation, just as a crystal of sodium sulphate, dropped into a solution of the same, will serve as a centre of crystallization.

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[1] This term is used for both the "hæmatoblasts" of Hayem and the "blutplättchen of Bizzozero, which are identical.

If a cover glass be firmly fastened to a slide with parafine, as directed by Hayem, and blood dropped at its edge, it will run beneath by capillarity, and in the islets free from red corpuscles may be readily seen, with a power of from 400 to 500 diams., the bodies described as blood-plates. They are exceedingly delicate, colorless bodies, with a diameter from two to three times less than that of the red corpuscles, and, according to Hayem, exist in the blood in health, to the number of 255,000 per cubic millimetre. They are scarcely under observation than they begin to alter in shape in a variety of ways, but in each instance a division of the blood-plate into two portions may be detected—namely, the one central, vitreous-like, and strongly refractive ; the other peripheral, granular, and of a gray color. The latter is extremely viscous, and causes the formation of groups of blood-plates, from the borders of which may be seen to proceed a number of exceedingly fine filaments that cross each other in every direction, thus forming an irregular network. These changes are exactly coincident with the formation of a coagulum, and the agents which prevent these alterations in the blood-plates are precisely the ones that prevent the formation of fibrin. Among these may be mentioned a temperature of O° C., certain saline solutions, liquor amnii, and dropsical effusions.

Hayem does not absolutely claim that fibrin is derived directly from his " hæmatoblasts, " for, in referring to the viscous matter that exudes from them, he propounds, and does not answer, the question whether it is actually fibrin or a substance that combines to form fibrin with some other substance held in solution by the plasma.[1] He holds that a fibrinous reticulum may exist without having the "hæmatoblasts" as points of departure, but maintains that fibrin is never formed without the participation of a substance derived from these very vulnerable elements. The views of Bizzozero are almost identical with those of Hayem, but, in addition, he has demonstrated that the blood-plates are the chief agents concerned in thrombus formation. He also concedes that the white corpuscles may be concerned in fibrin formation.[2]

The foregoing pages contain a *résumé* of our knowledge concerning the coagulation of the blood, and although the subject is still surrounded with some obscurity, it is manifest that there are excellent grounds for regarding fibrin as a metamorphosis of certain formed elements of the blood—namely, the white corpuscles and the blood-plates. The question whether the blood-plates are disintegration products of young red corpuscles, as claimed by Norris, is an entirely separate one.

In the opinion of the writer, a blood-clot is the result of two distinct processes: coagulation proper, and fibrin formation; the one chemical, the other morphological. Coagulation occurs in milk under the influence of a ferment (rennet), but there is no fibrin formation. The same process takes place in blood under the influence of a ferment derived from the white corpuscles and perhaps also from the blood-plates, while, at the same time, and IN ADDITION, there is formed a fibrinous reticulum which is a direct metamorphosis of the same elements.

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[1] "Est-ce déjà de la fibrine ou bien une matière en voie de transformation et se combinant avec une autre substance dissoute dans le plasma?"—*Arch. de Phys. Norm. et Path.*, tome v., 1878.

[2] "Die übrigen Versuche sprachen zwar viel mehr für die coagulative Rolle der Blutplättchen, als für die der weissen Blutkörperchen, machten aber letztere noch nicht in so hohem Maasse unwahrscheinlich und liessen namentlich die Betheiligung beider Elemente noch mehr order weniger denkbar," Ueber einen neuen Formbestandtheil des Blutes und dessen Rolle bei der Thrombose und der Blutgerinnung.—Untersuchungen von Prof. Dr. Julius Bizzozero, *Virchow's Archiv*, Bd. 90, p. 323.

NEW BOOKS AND INSTRUMENTS.

The Elements of Physiological and Pathological Chemistry. A hand-book for medical students and practitioners. By T. CRANSTOUN CHARLES, M.D., Lecturer on Practical Physiology, St. Thomas' Hospital, etc. Philadelphia : Henry C. Lea's Son & Co., 1884.

This excellent hand-book, though written by an English physiologist, seems to have made its first appearance under the auspices of American publishers—as there is no evidence that it is a reprint of an English publication. The precise scope of the book may be perhaps best understood by comparing it with the physiological chemistry of Gamgee, of which the first volume—equal in size to the entire treatise of Dr. Charles—appeared in 1880. The aim of both authors is to present to their readers, not merely a summary of facts and opinions in the important sciences of physiological and pathological chemistry, but to exercise them in methods of personal investigation. The very attempt to do this testifies to the progress which has been made in the exact study of the changes which take place in the living body. This progress tends to render physiological chemistry as "practical" an interest with the physician as are the "dismal" anatomical sciences whose material is only obtainable through death.

Dr. Charles' manual divides the subject under four main heads : Nutrition and Foods ; Digestion and the Secretions Concerned ; The Chemistry of the Tissues, Organs, and Remaining Secretions ; The Excreta, the Fæces, and Urine.

Gamgee's first volume is devoted to the chemical composition of, and the chemical processes relating to, the elementary tissues of the body, including the blood, lymph, and chyle ; while the chemistry of the chief animal functions is reserved for the second volume. The chemical changes involved in the activity of the nerve and muscle tissues are discussed with them, so the functions treated of apart must be those of digestion, nutrition, and excretion, which occupy as large a portion of Charles' manual. In this, the description of the chemistry of nutrition is preceded by chapters giving minute directions concerning the apparatus and methods employed in solving the problems proposed. Then the intimate nature of nutrition is described, and brief statements given of its relations to the evolution of force—*i. e.*, heat and kinetic energy. After a classification of foods and the quotation of several well-known analytical tables of their ultimate constituents, the different classes of food are described in detail,—the alcohols and fatty acids, the carbohydrates and especially glycogen and grape sugar, the fats and the albuminoids. Methods of examination for grape sugar are described in the first book, and also to some extent in the fourth, in the chapter on diabetic urine. Similarly, methods for the investigation of the albumens are described with great fulness in the first book, together with their derivatives, leucin and tyrosin, and tests for the amyloid degeneration ; while, in connection with the practical rules, the theories of the functions of proteids are succinctly summarized. Then the examination of albuminous urine and the pathology of albuminuria are taken up in the fourth section.

An interesting chapter on ferments, fermentation, and putrefaction, opens, somewhat unexpectedly, the section on digestion. The chemisty of the tissues to which the third book is devoted, is largely occupied with the blood, its gases, hæmoglobins, corpuscles, coagulation, chemical analysis and separation of its different constituents, and finally its pathology. The study of muscle is not nearly so extensive, it takes up but a single chapter, thus contrasting with the learned dissertation on the contractile tissues, which is a conspicuous feature of Gamgee's treatise. But the latter includes elaborate discussion on the physiology of muscular contraction, which is only briefly touched upon by Dr. Charles.

The chapters on the nerve tissues and on respiration are chiefly of theoretical interest : while, conversely, the practical and minute rules given for investigation of the excreta cannot fail to be useful to the physician. This chapter includes careful descriptions of biliary and urinary calculi ; and terminates with an admirable syllabus of a short practical course in physiological chemistry, such as really should be traversed by every student in medicine. In this course the student first examines a few hydrocarbons—starch, dextrine, glycogen, and grape sugar,—then passes to nitrogenous bodies, prepares and tests albumen, separates albumen from its solutions, prepares acid and alkali albuminates, fibrinoplastin, fibrinogen, syntonin, leucin, and tyrosin ; among the digestive juices, studies the saliva, gastric juice, pancreatic juice, and bile. The rules for the study of blood and muscle are much less numerous ; the first referring only to the corpuscles and coloring principle, the second to muscle extractive. The rules for the examination of urine are, however, complete.

Dr. Charles' manual admirably fulfils its intention of giving his readers on the one hand a summary, comprehensive but remarkably compact, of the mass of facts in the sciences which have become indispensable to the physician, and, on the other hand, of a system of practical directions, so minute that analyses often considered formidable may be pursued by any intelligent person who takes the necessary time and procures the necessary apparatus.

[M. P. J.]

A Practical Treatise on Fractures and Dislocation.—By FRANK HASTINGS HAMILTON, A.B., A.M., M.D., LL.D., late Professor of Surgery in Bellevue Hospital Medical College, etc., etc. Seventh American edition, revised and improved. Illustrated with 379 wood-cuts. 8vo, pp. 1005. Philadelphia : Henry C. Lea's Son & Co., 1884.

For twenty-five years this classical work has held first rank among practical treatises in medicine, and each edition has kept pace with the times. As our readers are already familiar with the work, we will confine ourselves to a reference to a few of the points which mark the present edition.

Dr. Hamilton has seen fit to retain in the present edition the chapter, entire, on "General Prognosis," as it first appeared in the edition of 1880. The greater part of this chapter is devoted to the discussion of Dr. Sayre's "Report on Fractures," made to the American Medical Association in 1874, which argued that, with extension and counter-extension made in the proper direction, the perfectly normal condition of the bone as to length and position could be obtained, and so held until consolidation. This doctrine of practice was seen at once to be dangerous, because, on insufficient grounds, it ran contrary to the experience of the profession. It is here shown that Dr. Sayre's conclusions were founded on imperfectly recorded cases, and that he made no use of personal observation. We are led by the Report to believe that it was written in advocacy of plaster-of-Paris—not of the truth. It will hardly be necessary to cumber future editions of this book with this argument.

In the case of ruptured arteries in compound fractures objection is made to the recent proposition of Poinsot, to ligate the main arterial trunk which supplies the injured limb, or to compress the point of lesion and the main artery at the same time, because such procedure " would render the occurrence of gangrene almost inevitable," and " expose the life of the patient to greater danger than to amputate the limb."

The author's further experience adds testimony to the incompetency of Listerism to do all that its extreme advocates claim for it. In the treatment of compound fractures he gives carbolic acid the rôle of a mild stimulant, advocates cleanliness, makes it essential to avoid rudeness and officiousness, recommends the use of the drainage-tube when necessary. He says: " I do not believe—indeed, from actual experience I know—that the knee-joint cannot be 'freely laid open' under the Lister treatment 'with the certainty that no danger will follow.' "

The " popular error in reference to refracture " at any period after four or six weeks, thereby to " materially add to the length of the limb," is referred to.

In fracture of the ossa nasi Dr. Mason's dressing by means of a transverse pin with a strip of rubber is given with an illustrative drawing.

In the discussion of the causes of Colles' fracture Dr. Hamilton reviews the opinions and repeats the experiments of Bouchet, Lecomte, and others, which, in the sixth edition, he by mistake attributes to Dr. Pilcher as original. He asserts that he is " far from being convinced that this classical fracture, occasioned by a fall upon the hand, is due exclusively to the action of the ligaments." The results of his experiments are summarized at length, but the observation is well made that the facts adduced are those in " dead-house experiments," and, on that account, were not subject to the same conditions which are present in the living subject. This fracture in the living subject is the " result of concussion, avulsion, and muscular action combined." Here we recall the preface to the work, where the author very aptly fixes the degree of importance which, in the study of fractures and of dislocations, attaches itself to experiments on the cadaver, to clinical observations alone, and to cabinet specimens unattended by their clinical history.

Considerable space is devoted to the treatment of fractures of the patella. As might have been expected, the full faith in the Lister method, which swept clean, invited the boldest interference with the knee-joint in cases of fracture of the patella. Whether by wire, by silk, or by bone, the securing of the fragments of the patella in position, even with the Lister system, is shown by the testimony of Chauvel, Le Fort, Kocher, and others, to be attended by "tedious recovery, anchylosis, amputation, or death." And, for such reasons, the author condemns "washing of the joint," opening the joint to evacuate blood, and aspiration.

In the chapter on "Gunshot Fractures," we meet the familiar pictures, to show the injury which the rib and the vertebræ sustained in the case of President Garfield. The accompanying text gives briefly the clinical history and the results of the post-mortem examination.

The book has but few defects. We would prefer an arrangement in such books as are used for ready reference, as well as for study, whereby the multitude of references and opinions might be more readily distinguished from the more practical conclusions of the author and directions in practice. [J. V. D.]

A Manual of Obstetrics. By EDWARD L. PARTRIDGE, M.D. Wm. Wood & Co., 1884. 16mo, pp. 295.

This little book is a very good sample of its class. In these days, when even Scott's novels are issued in abbreviated editions, what wonder that a great subject like obstetrics should be summed up in about four hours' reading? The general arrangement of the volume follows the accepted text-books, and shows throughout that condensation of these has been done by one who is both a practical obstetrician and a class-room teacher. The best portion of the book strikes us as being the account of labor and its management, while the least satisfactory is the sketchy anatomical portion.

To the wider question of the real value of such a booklet, we must say we cannot rate it highly.

So long as our present system of medical education continues, when a student can pass an examination in obstetrics without ever having seen a case, such books as this will be in demand. They are useful to the student to cram upon, but for purposes of true and working knowledge they are not worth much. Such knowledge can only be acquired by seeing actual cases, supplemented by a larger and more philosophical treatment of the subject than the dimensions of this book allow. If these little manuals and remembrancers are of value in medicine, it is chiefly in those branches, like anatomy, where they serve to recall definite facts, and not in an eminently physiological branch like obstetrics.

The sixty illustrations, of which the title-page proudly tells us, are quite remarkable for their badness, truly a "making of darkness visible." [W. M. T.]

Diseases of the Rectum and Anus. By CHARLES B. KELSEY, M.D., Surgeon to St. Paul's Infirmary for Diseases of the Rectum, Consulting Surgeon for Diseases of the Rectum to the Harlem Hospital and Dispensary for Women and Children, etc. Pp. 416. Wm. Wood & Co.

The arrangement of the book is excellent. An introductory chapter devoted to anatomy and physiology is presented, without which any book on a special subject would be incomplete. It includes an excellent *résumé* of the subject-literature, particular attention being paid to the discussion and expulsion from belief of the existence of the third sphincter, on which so much has been written. A very complete chapter is devoted to malformations of the rectum and anus, in which imperforate anus is especially considered, with excellent illustrations borrowed from Mollière.

The primary operation at the perineum for imperforate anus is deprecated and preference shown for inguinal colotomy either alone or with an accompanying operation for the formation of an anus. The author shows a tendency to commend the inguinal incision in preference to the lumbar for cases of intestinal obstruction in general; the opening of the peritoneal cavity not being fraught with the degree of danger in times past supposed. Our own experience leads us to consider it an operation easier of performance than the lumbar incision, and one that results in much less annoyance to the patient as long as he may live, but in point of safety the two operations cannot be compared.

In the examination of a patient with disease of the rectum the author recommends certain gynæcological chairs. We have found Sargent's lounge and table superior to any chair yet invented. Its perfectly flat surface for use in the dorsal decubitus and its lateral inclination when the patient is in Sims' position render it especially useful in this class of diseases.

We appreciate the sense of the arrangement in sequence of abscess and fistula when we consider the etiology of the latter. In fact, the general arrangement of the book is excellent, and it covers a wider range of subjects than any book that we have heretofore seen.

The subject of fistula has not, we think, been given all the attention it deserves, and the same may be said of the chapter on hemorrhoids. Mr. Allingham, in his most excellent work, devotes one fourth of the book space to hemorrhoids, and an equally searching study is made of fistula. It adds greatly to the completeness of a book to enter elaborately into the study of the rarer forms of disease affecting a certain region, and Dr. Kelsey's work covers a wider range of subjects than any other book with which we are familiar, but for all readers of a work on diseases of the rectum, specialists as well as general practitioners, too much cannot be written on these two subjects. There is a tendency among the laity to consult charlatans and irregular specialists when afflicted with either of these diseases, and it is the duty of the physician to wrest these cases from them. It devolves especially upon the general practitioner to do this, and for his instruction particularly should a lengthy discussion of this subject be made.

More attention is given to rectal hernia than in any other textbook on this subject. There are no published accounts of any cases having occurred in this country, but the condition is interesting as a surgical curiosity at least.

The arrangement of the latter part of the book, which treats of new growths and rectal stricture, is excellent. The division of neoplasms, ulcers, and stricture into those that are malignant and those that are non-malignant, is proper and practical ; method and arrangement in classification by authors are greatly appreciated by the book reader, and, especially, by the searcher after individual subjects.

Dr. Kelsey is a warm advocate of the operation of external proctotomy as a substitute for colotomy in stricture of the rectum, and in that preference he is up to the times. We have noticed, especially in Europe, the growing abhorrence of colotomy and the increasing favor of external proctotomy in suitable cases, and they include a large proportion of those for which colotomy is generally performed. Excision of the rectum in proper cases, for cancer, is advocated by the author in preference to lumbar colotomy, and in his belief he is in accord with modern thought, especially among German surgeons, with Volkmann at their head. It has often occurred to us that the great sacrifice of life whic his sometimes made by our Continental neighbors in the development of a new operation cannot be accused of being *nil* in its results. Their experience and conclusions only serve to place the procedure on its proper footing and permit us to profit by them.

We do not object, however, to the school for experimentation being located abroad. [F. C. F.]

Beitrag zur Kentniss der Menstrualen Psychosen. By Ellen F. Powers, Zürich. 1883. [A Contribution to our Knowledge of Menstrual Psychoses.]

This excellent essay, which forms the thesis presented by its author to the faculty of the University of Zürich, treats of a form of insanity which, notwithstanding its practical and theoretical importance, has been somewhat neglected by alienists. This is probably due to the inherent insufficiency of a classification of insanity which is based upon purely symptomatic data. In accordance with this system most writers have been satisfied to group the various forms of insanity and psychic disturbance recurring in connection with the menses under the head of periodic and circular insanity. These forms, as a class, have been well studied, and whatever has been affirmed of the whole group is affirmed of the psychoses included under that group ; so that without much especial study of menstrual insanity, it has been assumed that since this is typically periodical in its manifestations, it shares with other periodical insanities their almost hopeless prognosis.

In one of the most important monographs upon this subject, that of v. Krafft-Ebing (Archiv für Psychiatrie, Bd. viii., p. 65, 1878), the eminent author reports nineteen cases observed by himself, and expresses the view that these menstrual psychoses are most typical forms of periodical insanity. The same author considers this as belonging to the group of degenerative insanity, of which the prognosis is eminently unsatisfactory. He believes, however, to have found in potassium bromide "the only remedy which can favorably influence the bad prognosis of the disease."

In the essay before us Dr. Powers has made an analytical study of nine cases of menstrual insanity which have come under her own observation. Of each of these we have excellent histories, especially admirable in their fulness as to points in their heredity, as well as in the completeness of the physical examination of each patient. There are frequent notes of the behavior of the patients during the time they were under observation (chiefly in the insane asylum at Zürich), together with the treatment pursued in each individual case. In addition to these nine cases of her own, the author has also collected sixty-eight cases from literature, old and recent. Many of these are necessarily incomplete, but in general furnish a rich store of material, upon which the author has based certain conclusions as to ætiology, diagnosis, and, above all, prognosis and treatment.

Dr. Powers "believes that periodic menstrual insanity cannot be properly classified as readily as would at first appear; for although it has many resemblances to the periodical and cyclical psychoses, such as the typical recurrence of similar attacks at regular intervals, the neurotic disposition and behavior of the patients during intermissions, and the abrupt onset of the attacks," it differs from these forms of insanity:

1st. In possessing a much more favorable prognosis.

2d. In the well-known ætiology of each individual attack.

3d. In the fact, which v. Krafft-Ebing also observed, that the consciousness of the patient is not as much affected in menstrual insanity as in the periodical and circular psychoses.

4th. In that, menstrual insanity is typically a "reflex psychosis."

As is well known, menstrual insanity manifests itself in a variety of forms; no two cases are exactly alike; they vary in severity as well as in character. In one patient we may have only slight and scarcely observable mental aberration at the time of the menses, while in another the insanity will manifest itself in a violent form of acute mania. The cases studied in the work of Dr. Powers are divided by the writer into the following groups:

A. Characteristic, well-marked cases of menstrual insanity.
    a. mania; *b*. melancholia; *c*. other acute forms.

B. Abortive cases of menstrual insanity.

C. Transition forms between periodic menstrual mania and mania simplex.

D. Characteristic, well-marked cases of circular menstrual insanity (mania at time of menses, with intervals of melancholia).

E. Transition forms of circular menstrual insanity combined with sexual perversion.

F. Stuporous insanity, cataleptic menstrual insanity, and insanity which takes the form of pathological drowsiness.

G. Hysterical attacks in connection with the menses.

H. Dipsomania menstrualis periodica.

I. Kleptomania menstrualis periodica.

All of these varieties are well-marked forms of insanity, with the exception of group *G*. The case coming under the group is one of hysteria pure and simple, with a menstrual ætiology for each of the hysterical attacks.

As to "differential diagnosis between other psychoses and periodic menstrual insanity, this depends upon the menstrual ætiology of every attack in the latter affection. In the typical cases, at least, it is positive that the same nervous irritation which causes the menstruation also gives rise to the attack. This irritation is probably that of ovulation. It starts from the periphery—that is to say, in the ovarian nerves, and passes through the ordinary reflex channels to the central nervous system, upon which it produces an intense impression. That this takes place, to a certain extent even in the normal state, is shown by the manifold physiological and pathological changes which occur in the disposition and character, as well as the vaso-motor functions of normally healthy women at the time of menstruation. Periodic menstrual insanity, therefore, is only the exaggerated result of this normal irritation."

Dr. Powers' experience, as well as that of v. Krafft-Ebing, is that most patients affected by this disease "have a neuropathic constitution, and are of an hereditary neurotic dispositon. To this rule, however, there are exceptions, some of our patients presenting nothing abnormal, and possessing a good hereditary history." "In this connection we must not forget that even among psychically healthy people the percentage of those whose hereditary history shows a neurotic taint is in all probability relatively larger than one would suppose."

"v. Krafft-Ebing has called attention to the ordinary neurotic initial symptoms of menstrual insanity: there are headache, rush of blood to the head, increased or diminished frequency with irregularity of pulse, dizziness," etc. * * * "Ordinarily the attack is preceded by sleeplessness and irritability"; all of these symptoms may be wanting, however, and then the attack begins suddenly. "The termination of an individual attack may be either sudden or gradual; in every case the attacks resemble each other in general character."

An important point in the differential diagnosis between periodic menstrual insanity and an ordinary attack of mania, is the nature of the post-paroxysmal symptoms. In menstrual insanity the attack is followed by great mental and physical weakness and exhaustion, while ordinary mania gradually merges into a condition of great irritability, with a tendency to violent outbursts of temper, followed later on by the well-known post-maniacal, stuporous, or melancholic stage of the attack; the general physical condition of these patients is good.

In every case of periodic menstrual insanity we should be able to ascertain:

First, that the paroxysms are regular and evidently connected with menstruation—or, rather, ovulation.

Second, there must be a history of intervening clear periods.

Third, the attacks must not only consist of simple and slight disturbances, such as frequently occur in nervous women, but the mental disturbance must be well-marked.

There will, of course, be cases which do not clearly combine these three characteristics, and which yet seem to belong to the group of periodic menstrual insanity. Such are :

a. Cases in which the attacks are periodic, but which do not always correspond with the menstrual period, or in which the attacks are the result of an irritation other than that of ovulation (approaching other periodic psychoses in character). *b.* Cases in which there are no clear intervals, and where menstruation only causes an acute exacerbation of the disease (continuous psychoses). *c.* Abortive cases, in which menstruation regularly causes a neurotic disturbance, psychic in character, yet scarcely severe enough to be classed under insanity (resembling symptoms produced by normal menstruation in nervous females).

Dr. Powers believes that menstrual insanity need not necessarily be periodic in character : thus, there may be cases of menstrual insanity cured after the first attack ; or there may be cases in which the psychosis produced by the irritation of ovulation may last until the next period, when a new irritation occurs, thus precluding the occurrence of any intermission.

Dr. Powers does not agree with Krafft-Ebing, in considering the prognosis of periodic menstrual insanity as bad as that of other forms of periodic insanity ; the results obtained by the doctor would warrant a much more favorable prognosis. Of all the cases of which the author of this paper has reliable statistics, about seventy per cent. were cured. "Under proper and timely treatmeet, therefore, menstrual psychoses would appear to offer an extraordinarily good prognosis ; while in cases occurring later in life, the menopause may be expected to exert a favorable influence in their cure."

The chief method of treatment which the author followed, in nine cases treated by her, was the "preventive and symptomatic administration" of potassium bromide in proper doses ; but it is worthy of remark, that the author believes that good nursing, as well as hydrotherapy, exercise in the open air, starchy and fatty diet, and rest in bed, both before and during each menstrual period, have as much value in the treatment as the administration of drugs. [H. W. B.]

Chlorate of Potassa. Its physiological, toxic, and therapeutic action. Dr. J. VAN MERING, Docent at the University of Strassburg. Berlin, 1885.

This elaborate monograph opens with an extensive historical review of previous researches upon chlorate of potassa. This review begins with the year 1797, when Foucroy, inspired by Lavoisier's researches on respiration, developed his theory that nitric and chloric acids and their salts acted in the organism by yielding up to it the large percentage of oxygen they contained.

The first application of this theory was made by a pupil of Foucroy's, Alyon, who in the fifth year of the French Republic, used chlorate and nitrate of potassa in the treatment of syphilis. With about as much reason, these salts became celebrated in several other diseases for which an internal supply of oxygen was supposed to be desirable—thus, scurvy, diabetes, typhus fever. The most famous illustration of this peculiar theory, which was accepted without a pretext of experimental demonstration, is the advice of Simpson to treat women liable to abort, by chlorate of potassa, in order to furnish oxygen to the fœtus when the placenta was degenerated. Virchow considered this recommendation plausible. The other most famous application of chlorate of potassa has been for ulcerative, gangrenous, and mercurial stomatitis. Its utility in these affections was first asserted by Hunt in 1843, as the result of twenty years' experience. A great many physicians added their testimony to the same effect, and the monographs of Bergeron and Isambert (the latter in 1856) became influential.

On the basis of this clinical experience in stomatitis, the salt was tried in diphtheria, and as late as 1877 Seëligmüller enthusiastically recommended it as a specific for this disease. More recent observation has limited the belief in its efficacy. But it has confirmed its value as a remedy for hyperæmia of the buccal and pharyngeal mucosa ; and, on the other hand, has suggesting new dangers from the irritative effect of the salt upon the kidneys during elimination.

Mering reviews the researches hitherto made upon the physiological and toxic action of chlorate of potassa, and then proceeds to describe his own. The first chapter of these touches upon the elimination of the salt, and the methods of detecting it in the urine; the second treats of the influence of the chlorate upon albuminous metabolism ; the third, on the alterations of the blood through chlorates and free chloric acid ; the fourth investigates the nature of the substances which may reduce chlorate of potassa ; the fifth inquires into the anti-putrefactive properties of chlorates, bromates, and iodates ; and the sixth describes the action of bromates and iodates upon the blood.

The determination of chlorate of potassa in the urine is important in the solution of the question whether the salt is really reduced in the animal organism, as is assumed in so many therapeutic theories. It is necessary to be able to recognize the chlorate in the presence of chlorides and perchlorates. Mering effected this by the following method. In one portion of urine the chlorine of the chlorides was directly determined by the ordinary process of decomposing the latter with nitric acid, precipitating with nitrate of silver, weighing the precipitate, and calculating from the atomic weight the amount of chlorine it contained.

In another portion of urine, the chlorates were reduced to chlorine by heating for an hour upon a water-bath with powdered zinc and diluted acetic or sulphuric acid. In the filtrate the amount of chloride was then estimated by the usual method. The difference in the result before and after reduction with zinc indicated the amount of chlorate.

The following experiment illustrates the method. Twenty c. cm. of urine yielded, after dilution and on the addition of nitric acid and nitrate of silver, 0.373 grm. of chloride of silver. Twenty c. cm. of the same urine, to which 0.5 grm. of chlorate of potassa had been added, gave, after boiling with powdered zinc, 0.954 grm. of chloride of silver. The difference is 0.581 grm., which is derived from the reduced chlorate of potassa. Theoretically, 0.5 grm. of the chlorate should have yielded 0.585 grm.; thus the practical difference was not great.

Experiments were then made upon healthy human beings, upon dogs, and upon a syphilitic patient, to all of whom chlorate of potassa was administered internally. The following results were obtained :

	Ingested.	Eliminated.
Healthy man . .	5 grms. in 5 hours.	4.62 in next 24 hours.
" " . .	3 grms. at dose.	2.7 " " " "
" " . .	1 grm. " "	0.91 " " 10 "
" " . .	0.05 grm. " "	Traces after an hour.
Syphilitic woman with mercurial stomatitis .	5 grms. in 12 hours.	4.54 grms. in 24 hrs. in urine and saliva.
Dog	15 grms. at dose.	14.5 grms. in 24 hours.
"	20 " " "	Death the next day.

These results agree with those of Rabuteau and Isambert, who also recovered in the urine almost all the chlorate ingested.

Still there is a partial reduction, which is marked in proportion to the amount of the salt ingested.

The influence of chlorate of potassa upon the albuminous metabolisms of the organism was subjected to a second series of experiments. By means of the Voit method, a dog was brought into the condition of "nitrogenous equilibrium," and then chlorate of potassa administered, and its effects upon the elimination of urea estimated. After a dose of twelve grms., a dog that on two preceding days had excreted 610 c. cm. urine with 70.02 grms. urea, and 580 c. cm. with 72.04 grms. urea, excreted 770 c. cm. urine with 85.3 grms. urea. The proportion of both urine and urea returned to the original standard on the following day. The chlorate was shown thus to act as a diuretic analogous to chloride of sodium, acetate of potassa, and other neutral salts. The author infers that an increased decomposition of albumen also takes place. But the results may be equally interpreted as indicating an increased outstreaming from tissues of the urea formed in their elements from the albumen circulating through them.

The third series of experiments examined the alterations of the blood which might be caused by chlorate of potassa. The general effect was first tested by mixing defibrinated ox blood with a five-per-cent solution of chlorate of potassa. The blood immediately assumed a clear red color, which passed in two hours to a reddish brown. Examined in the spectroscope the mixture showed two oxyhæmoglobine lines. After four hours these were replaced by two methæmoglobine lines (between D and F). After twenty-four hours the mixture was coagulated, brownish black, and gave the hæmatine spectrum.

Similarly, chlorate of potassa, added to a concentrated solution of oxyhæmoglobine, formed brownish-black masses ; and we may infer that a prolonged action of chlorate of potassa on the blood results in the formation of hæmatine. The alteration proceeds much more rapidly with a warm than a low temperature, and is proportioned to the amount of the chlorate used. At 22° C. 0. 2 grm. and at a temperature of 37° C. 0. 1 grm. of chlorate sufficed to generate methæmoglobine in 100 c. cm. blood within twenty-four hours.

When the blood was first mixed with a large quantity of CO_2 the addition of chlorate of potassa altered it much more rapidly than was otherwise the case. Dyspnœic blood was altered twice as fast as arterial blood. The blood was obtained from the carotid of the same animal in both cases, but in one dyspnœa was excited by compression of the trachea. The alteration of the hæmoglobine was again greatly facilitated by even a slight diminution in the alkalescence of the blood (effected by the addition of a little acid phosphate of soda). On the contrary, the alteration was much retarded by the addition of a trifling quantity of caustic soda or carbonate of soda.

It is difficult to estimate how far these laboratory experiments may be taken to indicate changes in the blood by the internal toxic, still less by the internal therapeutic, administration of chlorate of potassa.

Analogous experiments were instituted to ascertain whether during the alteration of the hæmoglobine the chlorate of potassa suffered any change. Examination of the blood in which methæmoglobine had formed proved a partial reduction of the chlorate to chloride, without any simultaneous formation of perchlorate. The following are some of the results :

Mass of Blood.	Temp.	Duration.	$KClO_3$ Used.	$KClO_3$ Reduced.
100	22° C.	6	5,00	0,170
"	"	20	5,00	0,480
"	"	24	0,050	Not reduced.
"	"	48	0,005	0,005
"	25° C.	30	5,00	0,89
"	37° C.	6	5,00	0,81

Thus the reduction, by the same temperature and duration of the experiment, is dependent upon the absolute quantity of chlorate ingested. Small quantities were completely reduced ; larger quantities always left a considerable, indeed by far the greater, proportion unreduced. This observation correlates those upon elimination, which showed a more abundant elimination of uncharged salt in proportion to the increase in the dose.

Normally, the author admits, in the circulation of a living animal, the reduction suffered by the chlorate is so insignificant that it may easily be overlooked. Thus there is no reason to suppose that methæmoglobine would form in the blood of a man of 65 kilos. with a blood mass of 5 kilos., who for several days should take a gramme of chlorate every hour. Yet this is certainly an excessive dose.

Lecithin, a constituent of the blood corpuscles ; sugar, a constant constituent of the blood ; fibrine, blood serum, were all tested as to their capacity to reduce chlorate of potassa. The results were negative. A watery solution of red blood corpuscles and a solution of pure hæmoglobine did, however, effect the reduction. It is to be inferred that the reduction observed after admixture of the salt with blood, or, to a much less extent, after its internal administration, is due to oxyhæmoglobine.

The blackish masses formed by the prolonged action of chlorate of potassa on the blood, resist putrefaction for a long time, and the same antiseptic action is exercised by bromates and iodates. Bromates also reduce hæmoglobine, but more slowly than chlorates, while iodates have no effect at all.

The chapter of the monograph which the author devotes to general conclusions considers at some length the elementary conditions in acute poisoning from chlorate of potassa, and the means of treatment. Finally we have a few words on the legitimate therapeutic applications of this famous remedy.

The most reliable indication for its use is still diseases of the buccal cavity, especially stomatitis mercurialis, but also for stomatitis ulcerosa ; while, according to the author, the chlorate is of doubtful utility in aphthous stomatitis, tonsillitis, and sprue. It is extremely useful in ozæna ; is without action in diphtheria in small doses, and dangerous in large, " because in diphtheria are present all the conditions which favor the toxic action of the chlorate on the blood, as dyspnœa, high fever, anorexia, nephritis." It should, therefore, be employed chiefly as a gargle.

The favorable action of chlorate of potassa upon ulcerations of the buccal and nasal mucosa depends upon the oxygen contained in the chloric acid. The carbonic and other acids present in the mouth gradually decompose the salt and oxygen is given off, which exerts a gentle but permanent caustic action on the ulcerations and finally effects their cure. This local action is of much greater value than any which can be obtained from the internal administration of chlorate of potassa—opinion diametrically opposed to that expressed by Isambert, but, from the facts adduced, not without plausibility.

Mering closes with an energetic protest against the popular use of chlorate of potassa, and the habit of freely prescribing it by apothecaries without a medical prescription. " There are few medicines possessed of such marked toxic qualities which are used as extensively as chlorate of potassa." In 1875 419 kilogrammes were used in the Paris hospitals. The author, with somewhat superfluous energy, denounces the use of certain pastilles (pastilles de Dethan), each of which contains 10 centigrammes of chlorate of potassa (about 1½ grains). A package of these pastilles contains 10 grammes (150 grains), and that might easily cause the death of a child. But when would such an amount be swallowed?

Many voices have recently been raised in warning in regard to the dangers of chlorate of potassa, especially the danger of nephritis. But we think the author's own experiments tend rather to show that such warning, however appropriate for large doses, does not really apply to the doses habitually employed—say 4 to 5 grains hourly, or 96 to 120 grains in 24 hours, if so many doses were taken (6 to 7 grammes in 24 hours).

The net result of these careful and elaborate experiments should be, on the whole, to greatly limit the use of a drug which upon the basis of slender analogies, has been almost vaunted as a panacea. [M. P. J.]

The Editor of the ARCHIVES OF MEDICINE *regrets to announce the discontinuance of the journal with the present issue* (*December*, 1884).

The experience of the past year has convinced him that the verdict of the times is in favor of periodicals appearing at brief intervals, able to furnish scientific material and news with the utmost promptness and in a fresh state.

These considerations affect contributors and subscribers alike, and contribute to the preference for weekly issues.

In spite of such disadvantages, the Editor has received the most generous support within the past year, as well as during the entire six years covering the life of the ARCHIVES, *and to these friends, subscribers, and contributors he begs to offer his appreciative thanks.*

It is the belief of the Publishers and Editor that the volumes of the ARCHIVES OF MEDICINE, *of which a limited number of sets still remain, will remain a collection of no little value.*

E. C. SEGUIN, M.D.

NEW YORK, *December* 20, 1884

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TO THE

ARCHIVES OF MEDICINE

VOLS. I. TO XII. INCLUSIVE

The names of authors of original articles, cases, and editorials are printed in SMALL CAPITALS.

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